

A *Gal 72e*
SKETCH
OF A PLAN TO EXTERMINATE THE
CASUAL SMALL-POX
FROM GREAT BRITAIN;

AND TO
INTRODUCE GENERAL INOCULATION:

TO WHICH IS ADDED, A
CORRESPONDENCE
ON THE NATURE OF VARIOLOUS CONTAGION,

WITH
Mr. DAWSON, || Professor WATERHOUSE,
Dr. AIKIN, || Mr. HENRY,
Professor IRVINE, || Dr. CLARK,
Dr. PERCIVAL, || Dr. ODIER,
Professor WALL, || Dr. JAMES CURRIE:

AND ON THE BEST MEANS OF
PREVENTING THE SMALL-POX, AND PROMOTING
INOCULATION, AT GENEVA;

WITH THE
MAGISTRATES OF THE REPUBLIC.

By JOHN HAYGARTH, M.B.
F.R.S. Lond. F.R.S. and R.M.S. Edinb. and of the American
Academy of Arts and Sciences,

IN TWO VOLUMES.

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MDCXCIII.

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SKETCH

OF A

PLAN TO EXTERMINATE THE

SMALL-POX.

VI. I. *A LETTER from BENJAMIN WATERHOUSE, M. D. Professor of Physick at Cambridge, in New England, and Fellow of the American Academy of Arts and Sciences, dated June 18, 1788.*

DEAR DOCTOR,

‘AS you think the regulations, adopted at Rhode Island to prevent the spreading of the small-pox, afford a most important argument in favour of

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the doctrine you wish to establish, I seize the first opportunity of answering your further queries on that subject.

‘ I believe, with you, that “ the
“ use of the tremendous coffin, and
“ the transportation to an island, unnecessary and even pernicious ;” yet we do not think it prudent to abate any of the other regulations. The citizens of Boston and the people of all the other sea-ports use the same precautions. They send the infected, in a convenient carriage, to some sequestered place. Almost all the sea-ports have a small-pox hospital, built on some point of land, or at a considerable distance from the public road, on which a flag is displayed whenever it is occupied by the sick : so that, excepting the box, and the transportation by water, the regulations of Rhode Island, are the regulations of all New England.

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' The physician, who attends a small-pox hospital, never enters it, without taking off his wig, or covering his hair with a cap, changing his shoes, and having on a gown, which covers him from his throat to his ancles. When he comes out, he washes his hands, and fumigates himself with frankincense. However troublesome this may appear, it is so constantly practised, that one of the oldest and most experienced physicians we have in this disorder, tells me he never omits it.

' I cannot believe the precautions we use altogether unnecessary. We have tried many experiments with the small-pox in New England, and persuade ourselves, that we have some pretensions of knowing more of that disease than you in Europe.

' Our towns are small, our houses scattered, most of them having a gar-

den between them, so that we have been able to trace the influence of the contagion, from its first entrance to its expulsion. Hardly any person is taken with the small-pox, without its being known by the publick, when, and how he was taken, where he lives, and how he does from day to day. I could send you such accurate lists, kept by clergymen of small-pox patients in their own parishes, as to convince you, how very closely we observe this contagion, and how priest, magistrate and people conspire to stop the ravages of this disease. I doubt whether the fear of dying by the disorder, or of the abridgment of liberty, to which a family voluntarily submits when infected, operates most strongly.

‘ We think our precautions necessary, and justified by experience; and by way of vindicating them, I shall relate

relate some well attested instances; which are, however, rather opposite to some of your conclusions; but as I am convinced, the aim of both of us is one and the same, the discovery of truth, and our intention the same, namely to apply the truths we discover, to the good of 'our fellow-men, I shall relate what I have collected and believe to be true, and willingly leave the decision to an older and a more experienced person.

I. ' The first of the two prevailing opinions you notice is, "*that clothes, &c. exposed to the various effluvia, are supposed to retain an infectious quality.*" This opinion is universally received here; and I could give you a number of instances to prove that it is well founded, related by old clergymen, physicians, and distinguished literary characters.

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‘ Dr. Brattle, after visiting patients in the small-pox, used the common precaution of covering his clothes with a loose gown, &c. but neglected his wig. In consequence of this small neglect, after riding six miles on horseback, he gave the disease to a person in a room, through which he passed, where he did not stay to sit down.

‘ David Anthony, Esq. one of the overseers of small-pox in Rhode Island, after going into the small-pox hospital, and using the common precautions, neglected to smoke his wig. In his way home, two miles from the hospital, he called at the house of his daughter. He did not dismount, but sat on his horse, and talked to her through an opened window; and at the common period (by which we usually understand about fourteen days) she took the disease and died.

‘ Many

‘ Many such instances could I relate where wigs have given the infection, after being exposed to the open air during the passage of several miles. I therefore give credit to the story of the medical wig, mentioned in your *Inquiry* ;* not because my venerable kinsman and patron Dr. FOTHERGILL related it, but because I have heard so many well attested accounts in my own country nearly similar.

‘ Several old physicians, whose names it would not be prudent to mention, have told me in confidence, that they were conscious of having given the small-pox to some people, by neglecting, in the hurry of business, some precautions, which you in England are apt to disregard.

* p. 4.

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‘ In regard to the *infection adhering to wooden furniture and the like*, I shall mention the following fact. A servant-maid took the small-pox from washing a floor with hot water two years after there had been any small-pox patient in the room.

II. ‘ With respect to the other popular opinion, namely, “ *when the distemper is epidemical the whole atmosphere of the place is thought to be contaminated* ;” I would observe that that physician who believed the variolous infection to extend *thirty miles*, and he who supposed it to extend not *thirty yards*, seem, according to our observations, to be equally out of the way. Charlestown is separated from Boston by a deep river *fifteen hundred feet* wide. Close by the water side in Boston, was a house or two, infected with the small-pox ; in one of those dull foggy days, (such

(such as commonly produces a dark day in London) when there was scarcely wind enough to blow the smoke from the tops of the chimneys; yet what wind was stirring, wafted it across the river to Charlestown. In its direction, on Charlestown side, was a ship yard, and ten, or a dozen carpenters at work, all liable to the disease; they all took the small-pox, excepting two, and the eruptions appeared at the usual time from that day.

‘ We have the clearest proof of the small-pox being communicated to the distance of several hundred rods, viz. from the hospital at West Boston to several families in the neighbourhood. The atmosphere was for many days loaded with aqueous vapours, and the little motion it had was towards the houses where the small-pox made its appearance. The guard (for the hospital

hospital was military) which was continually kept, prevented all communication.

‘ In 1776, the small-pox broke out in the American army, then encamped in this town of Cambridge; and almost all that body of men were inoculated under the direction of Dr. Rand, an experienced and respectable physician, who can remember when inoculation was practised by Dr. Boylstone, who, in 1721, first introduced the salutary art into America. You can form some judgment how few had previously passed through the small-pox, when I tell you that Dr. Rand inoculated in a few weeks twenty-nine practitioners of physick; and yet it was so managed that the disease did not spread through the country. This experienced physician has read your book, and given me his remarks; he is firmly of opinion, that the sphere
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of infection is much wider than you suppose.

‘ A notion universally prevails in this country, *that the small-pox is most readily communicated by burning infected clothes.* Of several instances, related by my medical friends, take the following from Dr. Rand. A certain governor of this province arrived at Boston from England, and one of his servants had the small-pox during the passage. The vessel anchored in Charles-River; and the governor, knowing the dread, which the people he came to live among had of the small-pox, politely submitted to every restriction, like a private man. All the bedding, furniture, &c. used by this servant, were carefully brought on shore and burnt in a field near Charlestown; the people living directly in the wake of that smoke were attacked with the small-pox, and the disease spread. This is
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one of those periods, mentioned by Dr. Douglass.* The small-pox was not in the country when this ship arrived.

At a time, when the disorder had not been in the country for several years, a vessel arrived at Charlstown from Lisbon, laden with salt, and lemons in boxes. A person had the small-pox on board during the passage, and of course the small-pox officers took charge of the vessel, when she came to the wharf. They would not suffer the lemons to be sold, without being first unpacked, and the paper surrounding each lemon taken off. These papers were kept in a store house by themselves several weeks; and, after this, by order of the overseers, they were brought out and burnt; when, of two children playing round the fire, one named

* See INQUIRY, p. 15.

Manning,

Manning, took the disorder, and broke out at the usual time.

‘ These and the like accounts you will perhaps say prove nothing. But what shall we say to a great number of similar instances, which extort the same conclusions from a whole community ?

‘ It is said in page fifty-sixth of your INQUIRY, “ *What other fluids, that are contained in, or discharged from the body, may be deemed poisonous, is a question I believe that has never been considered, much less determined.*”

‘ On this head, take the two following instances : Dr. Rand was called to the wife of Col. B—le, whom he found hot and feverish, with a violent pain in her head and back ; but he had no suspicion of the small-pox. He bled her, and one
Mrs.

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Mrs. Brandon held the vessel to receive the blood, some of which spirted on her hand and arm. Next day the small-pox appeared on the Colonel's Lady, and she was of course immediately separated from Mrs. Brandon; notwithstanding, in twelve or fourteen days Mrs. Brandon was seized with the small-pox and died. It should be remarked also, that there were several persons about the Colonel's Lady, liable to the infection, yet no one took the disease but this woman, who stood over the blood, while it was running, and received some on her arm; except Mrs. Benjamins, to whom the basin of blood was handed, over the bed, who also took the small-pox from the effluvia of the blood, and died.

The other instance is still more curious. The same physician was called to the child of Manning, (who was supposed to have taken the small-pox from

from the burning of lemon papers as before related). He found the child bleeding at the nose in its mother's lap, who was then in the ninth month of her pregnancy, the next day the small-pox appeared on the child, and it was of course immediately separated from its mother, and all the family; nevertheless, in about fourteen days, the mother was seized with the disorder, and, not long after, delivered of a dead child, which child *had distinct eruptions over its whole body.*

‘ Would not these cases lead us to believe that the blood is capable of communicating the infection, before the disease is so far advanced as to be apparent on the surface ?

‘ Dr. Holyoke of Salem, who for his learning, professional abilities and integrity, is justly esteemed one of the first physicians in this country, and whose extensive practice has afforded

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him ample experience in the small-pox, writes to me, "that although he has reason to believe that an infected person seldom gives the disease, till after the eruption is considerably advanced, yet there are facts," says he, "which make it probable, that it is sometimes communicated earlier." The same celebrated physician adds, that your seventh proposition, with the inferences therefrom, rather contradicts all our opinions on this side the Atlantick.

' Upon the whole, we are led from experience to believe that *nothing less than a total separation of patients in the small-pox, and of all their attendants, from those, who are liable to the distemper, would be a perfect security from infection.* It is certain that *three hundred feet* distance, when the wind was favourable, has not been sufficient to prevent infection.

' It is an universally received opinion here, that of all known substances *cotton*

tion is the most tenacious of the variolous infection.

‘ We find no method of cleansing equal to *burying in the earth*. We do not trust to washing. A bed buried *forty-eight* hours, we consider as sufficiently freed from all infection. So much more efficacious is burying, than any other mode of cleansing hitherto tried, that had I an opportunity, I would actually try what effect a fresh sod of earth would have when applied to a limb loaded with pustules. That there is something remarkably *antiseptic* (if we may so speak) in the earth, I infer from this—when a rattle snake bites a dog’s nose, he digs a hole in the ground and lays his head in it, and it commonly cures him.

‘ The period between the infection and the commencement of the variolous fever, is commonly observed here to be, as with you, from the tenth to the

fourteenth day: but not always, for instance, in 1752, when the small-pox was epidemical in, and about Boston, the disease was mild, and generally commenced between the seventh and eleventh, and scarcely ever extended beyond the twelfth day.

‘ Although the facts I relate, and the general opinion on this side the Atlantick, are rather opposed to the doctrine you wish to establish, yet I scarcely dare say your observations are erroneous; because I know your habit of accuracy: I am moreover so agreeably impressed, not only with the practicability, but the benevolence of your design, that, did I think any thing I write would abate your ardour, I would put my papers in the fire. But I fear, lest an idea of the narrow sphere of the infection should mar so praise-worthy a design. I consider too that the prejudices of the people of England are against your doctrine, and possibly prudence

dence forbade you dwelling so largely on the facility of taking the disease as I have done.

After all, may we not both be right? May not the small-pox operate differently in the two countries? It has certainly had a different appearance, and required a somewhat different treatment almost every time it has come amongst us. That the difference in the virulency of the small-pox, observed at different periods, when epidemick here, may be attributed to a peculiar constitution of the atmosphere, no one seems to doubt; and why may not the difference, so reasonably to be expected between the atmosphere of your island and this continent, allow us to suppose that there is some difference in the facility of receiving infection? We know from a curious fact, related in the *Philosophical Transactions* for 1781, that during the blowing of the *Harmattan* the variolous infection could not

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be easily communicated even by art; for of seventy persons, who were inoculated, a day or two after the *Harmattan* set in, only one of them had either sickness or eruption. But after a few weeks, when the *Harmattan* ceased, the small-pox appeared among these seventy; about fifty of them were inoculated a second time with success, the others had it the natural way.

‘ In the year 1776, there were a considerable number of persons in Boston, to whom the small-pox could not be communicated by inoculation. In some the operation was repeated two, three or four times with fresh matter. Several of these persons have had the disease severely since, in the natural way, and some have died of it.

‘ A man who was a noted attendant in the small-pox hospital, and who had been employed several times when the disorder was epidemick, was at length,

length, to the surprise of his acquaintance, seized with the confluent small-pox and died.

‘ Have we not found that the small-pox rages with more malignity among some nations, than others? It has certainly almost extirpated the aborigines near the sea-coast of North America, though they have the advantage, from their mode of life, of fresh air, and no doctors.

‘ The following queries were sent to several of my medical friends, and their answers have been conformable to the doctrine advanced in the pages you have just read.

‘ (1.) What instances have you known of the small-pox being communicated by burning beds, clothes, &c. in the open air? and to what distance?

‘ (2.) What evidence have you of the contagion from a person's clothes,

wig, &c. in passing from an infectious room to distant houses of persons liable to take the small-pox? or to what distance do you think a person can communicate the disorder to another by the variolous effluvia, through the medium of the air, without ever touching him?

‘ (3.) Did you ever know a person take the small-pox from bleeding, or from holding the basin to receive the blood of a patient, a day or two before there was evidence of the disease by eruption?

‘ (4.) Is there any method of cleansing clothes equal to that of burying them?

Observations on Sect. III. p. 17. of the INQUIRY.
“ The variolous Poison is soluble in Air.”

The test is there said to be its *transparency*.

‘ (1.) Must we not, on this supposition, admit that the variolous miasms
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are *visible*, when uncombined with air? Is this supposition justified by observation?

‘ (2.) Are not the variolous miasms aerial, and consequently, from their exility, *invisible*?

‘ (3.) Can we, from the transparency of the solution of metals, earths, and other solid bodies, in particular menstrua, justly infer the chemical combination of two aerial and invisible fluids, because they retain their transparency when mixed?

‘ (4.) Would not such a combination (agreeably to a well-known law of chemical composition) destroy their chemical properties, consequently their peculiar smell and power of infection?

‘ (5.) May not the phenomena be accounted for, more easily, on a supposition, that the miasms are simply diffused in air? See p. 88 and 89.

‘ (6.) Is

‘ (6.) Is it not to their specifick levity, the consequence of their aerial exility, that we must impute the dissipation of variolous miasms, and other pestilential effluvia?—for the air near the ceiling of a room is, I believe, always most offensive.

‘ (7.) In p. 70, it is observed, “ that
“ the miasms are *perfectly* dissolved in
“ air, and consequently could not combine themselves with the clothes,
“ unless they had a stronger attraction
“ to the clothes than the air; in which
“ case they would remain in combination with the clothes.” In either case, must not their combination render them inoffensive? And if (as it is said in p. 88.) the whole air contained in the chamber of a patient, who has the malignant small-pox, is seldom, or never saturated, can there be any great hazard of infection in such a chamber?

‘ (8.) If

‘(8.) If the serum, pus, or scab, must be brought into contact with the person liable to the disease, or the effluvia must be received with the air as a vehicle, must not this, on the theory advanced in the *Inquiry*, destroy its activity, or power of infection?

B. WATERHOUSE.

2. ANSWER to *Professor WATERHOUSE*,
dated September 30, 1788.

DEAR SIR,

Accept my best thanks, for the obliging attention with which you have answered my questions concerning variolous infections. Your free and candid arguments merit high approbation and praise. I sought not your complaisant acquiescence in my opinions, but your unreserved doubts and liberal disquisitions. And as Englishmen, wherever situated, on both sides the atlantick,

Atlantick, will, I hope, always despise and reject any concession not founded on conviction, I cannot hesitate to return an answer dictated by an anxious wish to investigate the truth, which I doubt not, your candour will excuse and approve.

I. In my comment upon your excellent letter, concerning the measures of preventing the small-pox in Rhode Island, which was published as an appendix to the INQUIRY, I ventured to intimate, what I am still now more convinced to be true, that your salutary establishment was founded on fear, rather than knowledge. Your last letter confirms my idea, that the false notion of the infectious quality of clothes, &c. exposed to variolous miasms prevails universally. This opinion has been general in Europe, and your letter assures me, that no doubts concerning the truth of it have arisen in America. It would not have been difficult to collect

collect many facts among my medical and other acquaintance in England, which are generally supposed to contradict the doctrine I support. You may observe,* that I fairly admit the facts as sufficiently numerous and authentic, but deny the conclusion deduced from them. The cases detailed in your letter, are so clearly stated, and rest on such good evidence, that they appear to me peculiarly well adapted to illustrate this difficult and important question. You and most others, borne away with the prevailing hypothetical notions on this subject, will believe, that the histories you have collected clearly confirm them. For this reason, they deserve an accurate and impartial examination.

The cases you produce on the personal knowledge and experience of Dr. Brattle and David Anthony, Esq. are as circumstantially related as such nar-

* INQUIRY, p. 79.

rations can possibly admit. In the latter instance, the usual period of fourteen days is noted, which adds much authenticity to the observation. You infer, that a wig exposed to the variolous miasms, and afterwards to fresh air blowing through it, in one instance for six and in the other for two miles, still retained so pestilential a quality, as to communicate infection, at a greater distance and after a shorter interview, than, according to my experience, a patient in the distemper could have done.

I maintain, that a wig could not attract variolous miasms from the air, and again restore them to the air, on any known chemical principle. But, if you should not admit this doctrine with implicit confidence, yet you must allow that whatever poisonous quality is imparted to the wig, in the patient's chamber, would, on coming out of it, be communicated to fresh air by a constant

stant emanation ; at first, in greatest quantity and gradually less and less, till the whole has been exhausted. Consider how little poison could reasonably be expected to remain in a wig which had moved on horseback through six miles, or even two miles of fresh air, probably exposed to wind and weather. To illustrate this hypothetical notion, fancy to yourself one line of persons, susceptible of the small-pox, to be arranged closely to each other for two, and another for six miles. Your supposition warrants us to conclude, that these two wigs would have communicated infection to *twenty-one thousand one hundred and twenty* persons, that is, the former to *five thousand two hundred and eighty*, and the latter to *fifteen thousand eight hundred and forty*, if each occupied the space of two feet, the distance of soldiers in a regiment. As, on your supposition, the wigs were capable of infecting the last, it is manifest that they would be more capable of
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of infecting all the rest; unless you could suppose that they had a voluntary power of retaining and darting out the poison. Estimate, in your own mind, how many *million* of times less poison could be conveyed to any person by these wigs, than was insufficient to communicate the distemper in numerous instances faithfully reported in the INQUIRY, and the proceedings of the small-pox society at Chester.

In regard to the servant-maid who was infected by washing a floor; I only deny that variolous *miasms*, but do not dispute that variolous *matter*, may adhere to furniture. In cleaning the room, some infectious *matter*, that had been closed up from the air for two years, *might* be dislodged, so as to render the air around it infectious.

II. With respect to the other point of discussion; you will agree with me, that it is highly important to determine
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to what distance from the variolous poison the atmosphere is rendered infectious. You produce an apparently clear and strong fact to prove that this poison contaminates the atmosphere to the distance of *one thousand five hundred feet*, in the direction to which the wind blows. For ten or a dozen carpenters, liable to the disease, were all, except two, supposed to be infected at such a distance. No fact of this kind is capable of greater accuracy and authenticity. But consider attentively what would be the consequence, if variolous infection could contaminate the atmosphere, in any direction, to so great a distance. In the large towns of England, the disease is always present, and, at the same time, a great number of children, daily born, are every where dispersed near small-pox patients, and, though capable of infection, are many of them not infected, for many months or years. If the infectious influence were to extend *one thousand five hundred feet* to

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leeward of a small-pox patient, every person susceptible of the distemper would inevitably be attacked in a few weeks, in this climate of variable winds. Your dread of the distemper in America hurries all such patients to the pest-house, and prevents your having opportunities to ascertain the truth of this observation. But every town and every village in England can supply numerous facts, if duly observed, to warrant such a conclusion.

You lay much weight on the circumstance of a fog, as the means of spreading a contagion. Indeed, in Chester we have fogs so very seldom, that few occasions occur of making the comparison. But, on the eastern side of Britain, there are numerous fogs, as I have had frequent opportunities to observe, during my residence in London, Cambridge, and Edinburgh: yet no one in these towns has discovered, or even suspected, that infection spreads to a greater distance

tance in a foggy than a clear atmosphere. Such an appearance is well adapted to frighten the timid and ignorant, but no chemical or other philosophical quality of a fog affords any solid reason to convince a physician, that it can enlarge the pestilential atmosphere which surrounds a variolous patient. There is no occasion to suppose, that infection spreads by different laws in England and America. We have many advocates for your opinion on our side of the Atlantick, who produce facts which they think incontestably prove it. In the introduction to the INQUIRY, you may remember, that I quoted the opinion of an English physician, of the greatest eminence and knowledge, who had, from observation, concluded, that the infection of the small-pox extended, sometimes, thirty miles.

The fact you relate of the variolous infection being communicated from the

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military

military hospital at West Boston to some houses at the distance of several hundred rods, is as accurately ascertained as such a fact can possibly be. But the objections to the conclusion you deduce from it are still stronger in this than the former case; as I take for granted, that the distance here expressed is more than fifteen hundred feet: consequently the dilution of the poison would be greater, and the suspicion that the air at that distance may contain a sufficient impregnation to excite the distemper is proportionably diminished.

You may remember in the INQUIRY,* it is proved, in the circumstances there specified, that the variolous poison is diluted five hundred times more at the distance of forty-five feet than in the patient's chamber. How great then must be the *dilution* at the distance of *fifteen hundred feet*, or *several hundred rods*?

* p. 89.

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The smoke, arising from burning clothes which contained the variolous poison is supposed by MEAD on the Plague (P. II. C. ii.) to communicate infection to eight persons at a considerable distance. In my original manuscript of the INQUIRY this passage was quoted. But, on farther reflection, it appeared much more probable that infection had been conveyed to the eight persons, there mentioned, by some clothes, &c. secretly taken away after the patient's death. Burning of clothes seems more likely to destroy, than to propagate the poison. The Governor of Boston might order his clothes and furniture to be burned, but how could he know that such orders were punctually executed? Some sailor might convey on shore infectious articles. On the whole, washing or burning clothes seems the most effectual means to prevent all possible mischief from the variolous poison. If there should be any just suspicion of the vapour

arising from such a conflagration, a time and situation might be chosen, when the wind could not possibly convey it near people susceptible of infection. To burn infectious clothes seems a more certain method of preventing danger than burying them, as, at a future period, the poison, thus shut up from the air, might be dug up, and produce unsuspected mischief. I have not received the same conviction, which you express, of the purifying quality of fresh earth. To establish our faith in such a quality, the most direct and decisive experiments would be required; and a few such experiments would not be sufficient, if we may judge by the difficulty of obtaining a thorough confidence in the purifying quality of fresh air, though supported by numerous and authentick observations.

Your facts, that the effluvia of blood, drawn during the variolous fever, previous

vious to the eruption of the small-pox, has an infectious quality, are extremely curious, and as conclusive as any *affirmative* evidence can be. But I shall endeavour to shew, before the conclusion of this letter, the uncertain foundation of all affirmative evidence in these cases.

Is it recollected whether the variolous eruptions were farther advanced on Mrs. Manning, or on her child, at the time of its birth?

In all this reasoning, I hope, you fully understand, that I have never, for a moment, entertained the remotest suspicion of the truth and faithfulness of your narration. It is remarkably clear, distinct, and intelligent. Situations, distances, and respectable witnesses are accurately noted. Indeed few circumstances of consequence are omitted, except the dates of the stages of the distemper, and in some of the cases, the time when the eruptive fever

U 4 commenced.

commenced. These dates would probably have furnished remarks and objections.

You may allege, in reply to these arguments, that facts at Boston are placed against facts at Chester ; and that you have as good a right to draw your conclusions as I have to rely on my own. On this point I particularly solicit your attention, and wish you clearly to distinguish, that the *negative* proof is capable of being established by incomparably stronger evidence than the *affirmative* proof. Observation or experiment can determine, with much greater certainty, what *does not*, than what *does* give infection.

A distinct conception of this discrimination is required to comprehend the argument on which the INQUIRY is founded. It is calculated,* that, in any place visited by the small-pox, if *one*

* INQUIRY, p. 27.

has

has escaped the distemper, we may conclude, that it is nineteen to one he has not been exposed to the infection, unless we know the contrary. If *two* in a family have escaped, the probability that they were never both exposed is above *four hundred to one*: if *three* in a family have escaped, above *eight thousand to one*. By taking a greater number in a family or neighbourhood, who have all escaped the distemper, the probability that they have not all been exposed to infection, rises so high as to remove every possible doubt. This argument is founded on reason and common sense. When it first occurred to me, I attempted the above calculation of chances; but this branch of arithmetick having for a long series of years, given place to other pursuits, I applied to my mathematical relation and friend Mr. DAWSON. Weigh this argument attentively. If the calculation do not sufficiently impress your mind with a full conviction of
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of its truth and importance, try, as I have done, the experiment of peas,* until you feel its full force, and conclusive importance. A reader unaccustomed to this mode of argumentation, is apt to pass over such kind of logick, as vague and insignificant; as more adapted to puzzle than to convince; as more subtle and ingenious than true: but on serious and deliberate consideration, I trust, you will be fully and clearly convinced, that it is founded on reason and nature.

I am desirous to constitute and appoint you the advocate of my doctrine in America. By this solicitation, I do not mean that you should argue contrary to your own conviction. My request is simply, that you seek out and discover facts on the *negative* side of the question. These will be found the best supporters of my cause. In the accurate history of the progress of

* Described, p. 29 of the INQUIRY.

the small-pox, which tradition has preserved in America, is it not recorded that some patients have been far advanced in the disease before they were discovered : or, from their delicate or dangerous situation, were excused from going to the pest-house, and were allowed to remain at home during the most infectious state of the distemper ; surrounded, at no great distance, by several hundreds susceptible of infection, who were not infected ? Did the small-pox patients, close to the water edge in Boston, who were supposed to infect the carpenters in Charlestown, communicate the distemper to all the citizens of Boston, except *one in twenty*, situated within *one thousand five hundred* feet of the pestilential houses ? During the period infection remains in a family (from ten to sixty-three days*), did the wind never blow into the town of Boston from these houses ? The American and English winds are, I believe,

* See the INQUIRY, p. 57.

equally

equally variable, and, in such a number of days, generally blow round the whole compass.

Collect, I entreat you, all the facts you possibly can, to estimate what number have *escaped* infection, and at what distance, with minute accuracy. Such a body of evidence will be of great importance in determining the judgment of all physicians and philosophers who are capable of thinking for themselves. If you admit the principles, on which I reason, you will find no difficulty in discovering numerous cases to support them. The more numerous, accurate and authentick facts you can ascertain, the clearer will be your own conviction, and the more successfully you will convince others. But I would not flatter you with the hope of making many proselytes. Men, even those of enlightened understandings, seldom change their opinions, except a few of the younger class and
of

of superior genius, who can and dare correct the errors which they have imbibed in their education, from books and professors. By both titles, of youth and genius, I entertain sanguine hopes of your aid and assistance in the office I solicit you to accept, as advocate of my cause in America.

The conclusions which I am desirous to maintain, so essentially depend on this negative argument, that I am extremely anxious to explain and support it.

1. It is allowed by all medical practitioners, and fully confirmed by my own experience, that children, who have not been previously exposed to the variolous infection, are, on being inoculated, generally attacked by the small-pox. However, it is well known, that *all* do not thus take the infection. The excepted cases are computed to be *one in twenty*. If eighteen children, all

all born since the small-pox was in the neighbourhood, were inoculated by matter said to be variolous, collected and sent to me by another person; and if none of the eighteen patients caught the infection, I should conclude that the matter had not a proper or usual infectious quality: that the person who sent it me had been deceived or intended to deceive me. I should not think that the eighteen were incapable of receiving the distemper. It is contrary to all experience, that such a number together, should escape the small-pox, after being certainly exposed to the infection. This I call a *negative proof*, that all the eighteen were not inoculated with genuine variolous matter. This conclusion appears to be clearly deducible from reason and experience. If the expression of negative proof be improper, I shall gladly adopt a better.

The case here stated is not purely hypothetical. In a letter which I
lately

lately received from Dr. C. an ingenious physician at N. C. during a general inoculation executed in that town, after the example of Chester, he mentions that there were many disappointments in producing the small-pox by inoculation; that, in particular, *eighteen* were inoculated by matter from the country, and all remained uninfected. Dr. C. describes the matter as *mouldy*, which may satisfactorily account for such an unusual number of failures.

2. If the casual small-pox be introduced into a family, where several children are for the first time exposed to it, most of them catch the distemper, when no care is taken to prevent their intercourse. But in another family, if a plentiful eruption of a doubtful nature appear upon one, among several children, who intimately associate together, and if none of the rest become infected, it would constitute a *negative proof*, that the doubtful eruption was not variolous.

3. In

3. In determining the question, to what distance a small-pox patient renders the air infectious, I wish you attentively to consider the numerous facts related in the INQUIRY,* which are there circumstantially stated, and, in my opinion, fully authenticated.

These facts appear to exhibit *negative proofs*, that the open air is not contaminated to a great distance from the patient; not to one thousand five hundred feet, nor probably to one hundredth part of the space, in the circumstances there described.

4. When we investigate the question, whether the variolous miasms adhere to clothes, so as to convey infection to a patient, I am willing to decline the argument founded (though probably well founded) on theory, and merely trust the determination to facts. I can produce numerous instances, where

* § viii. from p. 93 to 108.

medical men, after exposing their persons and clothes to the miasms of an infectious chamber, in a short time nearly approached persons liable to the distemper, who are yet not infected, by this interview. English inoculators are daily in this situation, without communicating the casual small-pox.

If twenty, or ten, or even three children together, or in immediate succession, escaped infection, they would, in my opinion, afford a *negative* proof that they had not been exposed to the infectious influence.

5. Perhaps this argument may be illustrated by a case stated from another subject.

Suppose an apothecary had offered to send me some emetic tartar. If twenty patients were each of them to take a vomiting dose of it, as gr. ij. and if it excited no sickness in any of these cases,

X

I should

I should conclude that the drug which had been sent to me, possessed not the qualities of emetic tartar. Such facts I should deem a *negative proof* of such a conclusion.

Two grains do not excite sickness in every case where they are exhibited; but that twenty such cases should happen together is widely different from daily experience, and therefore utterly incredible.

This mode of reasoning, when the facts are sufficiently numerous, authentic, and accurately stated, impresses on my mind a degree of conviction little short of the effects of demonstration. I wish you freely to detect the errors or the obscurity of this argument. The question seems reducible to so simple a point that plain men, desirous to investigate the truth, cannot easily disagree; though ingenuity might start endless hypothetical subterfuges.

2. Compare

Compare the high degree of proof which the *negative* side of the question may admit, with the slender and dubious evidence, of which the *affirmative* side is capable. On one or more of the nine days, from the sixth to the fourteenth (or perhaps from the eighth to the sixteenth) previous to the commencement of the small-pox, the variolous poison was known or suspected to have been within fifteen hundred feet of the person infected, or a wig buffeted with the wind for two or six miles, may have approached such a person within a few yards. What proof, in this case, can possibly be produced that the variolous serum, pus, or scab, undistinguishable when separate from the patient, was not accidentally and ignorantly conveyed near the infected person?

You seem to think a military guard is a sufficient security; but soldiers do not search pockets, &c. with the dili-

gence of custom-house officers : and yet the variolous poison would even elude the interested search of the latter. This might happen through inadvertency in a thousand ways, as a wife, or comrade, might convey away a cap, or handkerchief soaked in the poison.

This method of communicating the infection is possible, and indeed highly probable. But the supposition that pestiferous air issuing from the doors, windows and crevices of a small-pox chamber can contaminate the atmosphere for several hundred roods, is wholly beyond the bounds of rational belief, and positively contradicted by thousands of negative proofs. Such an opinion has been suggested by fear and ignorance, without any rational examination. It will not bear a philosophical investigation, or a faithful comparison of facts.

Consider the numerous doubts and difficulties that would attend any decisive

five and conclusive experiment to prove *affirmatively* what does communicate infection.

The object of the experiment must be *shut up* in a house or box, and exposed to the poison on the sixth or eighth day of the confinement; but, before and after that day, cannot safely be allowed food, clothes, or any communication with the atmosphere, if we pretend to make it perfectly decisive. This is no exaggeration of the difficulty of such a conclusive trial. For washing, airing and fumigating would not be supposed by all perfectly to purify clothes. Baking and boiling might not be thought to cleanse sufficiently all food. A variolous scab might be sewed up in the lining of a coat; or thrust into the substance of various kinds of food; or *might* be brought to the orifices of the box through which the air for respiration is received.

If you were to begin such experiments with all these necessary precautions, and on the principles of the American creed, that the infectious influence extends to the distance of fifteen hundred feet; a long time and much patience would be required before you would arrive at the conclusion I have ventured to make; if my conclusion should happen to be true.

You may observe that the chief force of my argument rests on the accuracy of the observation, that not more than one person in twenty is naturally exempted from the small-pox.

The most eminent practitioners with whom I have conversed or corresponded on this subject (particularly Baron DIMSDALE) think this too large a proportion; he rather supposes one in one hundred nearer the truth; if so, my argument would become still more conclusive.

The

The third query of the INQUIRY * is proposed to determine this important point. It has been published about four years, and yet I have received no intelligence of any exceptions to the rule. Such silence is presumptive evidence that none exists; at least in such a degree as to invalidate the argument. Indeed, if the proportion were much greater; as *one* in *fifteen*, or even one in ten, the conclusion would be sufficiently cogent, especially when we may so easily discover not only two or three, but twenty or thirty in the neighbourhood of the poison, who have escaped infection. On this essential point, your letter very properly hints at some doubts.

1. You quote from the Philosophical Transactions, some very extraordinary effects of the *Harmattan* wind in Africa. It is there said to prevent the propagation of epidemics, as fluxes,

* P. 135.

small-pox, intermitting and remitting fevers, and to cure all disorders, even ulcers, that exist at the time of its commencement. This marvellous and even miraculous salubrity of the *Harmattan*, is related on the credit of the barbarous and ignorant negroes of Africa. But such miracles require better evidence to obtain rational belief.

These wonders are not even reported by a medical observer, but by Mr. Norris, a navigator, who indeed seems to have been curious and intelligent. The only circumstances he relates from his personal knowledge are, that he himself was cured of a fever, either by the medicines he took, or by the *Harmattan*; and that it prevented the effects of inoculation, an event that might with more probability be attributed to bad or effete variolous matter. But let this extraordinary event be ascertained how it can, or explained how it may, your candour will allow, on due consideration,

consideration, that it is not a case applicable to the question. Our business is to investigate in what manner the *casual* small-pox is communicated: it has no reference, whatever, to the mode of infection produced by inoculation. At any rate, this fact is not applicable either to England or America, as the Harmattan wind never blows in either of these climates.

The writer of the paper was the very ingenious Dr. DOBSON, with whose accuracy of discernment and excellency of understanding I have been long and very intimately acquainted. If the facts had been related on his own personal knowledge, no superior authority could possibly have been produced.

2. The noted attendant on the small-pox hospital, who had been often exposed to danger, without catching the distemper, may be allowed as one, among the few examples on record, of
a temporary

a temporary incapacity to receive infection. Such an event certainly does not happen oftener than once in several hundred or thousand instances.

Like facts are allowed to have occurred in the INQUIRY,* and it is there truly said, that they could not alter the proportion of *one* in *twenty* more than a small fraction, and would not materially diminish the force of the argument.

3. "In the Year 1776, there were
"a considerable number of persons at
"Boston, to whom the small-pox
"could not be communicated by inoculation; yet several of these have
"since had the natural small-pox."

Such disappointments are not uncommon. But, I believe, that they do not generally proceed from the incapacity of the person to receive infec-

* p. 26.

tion.

tion. The variolous matter may be bad, or imperfectly applied, or wiped off; as in the eighteen patients who were inoculated with mouldy matter above-mentioned.

With the same matter, and in the same patient, inoculation is commonly performed by two or more incisions. Some of these very frequently heal and denote no marks of infection, while others inflame, suppurate and produce the inoculated small-pox. These cases appear clearly to prove that the inoculator had imperfectly applied the poison to the incisions which did not inflame.

ANSWER *to the* OBSERVATIONS *on the*
THIRD SECTION *of the* INQUIRY.

Your theoretical objections, proposed under this head, are acute and ingenious, but in my mind admit of satisfactory answers.

1. The

1. The variolous poison is certainly visible, as it exists in scab, pus, or serum, though, in the last state, it is partly transparent.

2. There seems no more reason to think that the miasms of the small-pox are aerial, than of any other invisible odours, as of asafœtida, camphor, &c. or that the invisible solutions in water are aqueous, as, of salt, sugar, &c.

3. Two kinds of air, though substances of the greatest exility we are acquainted with, are visible, when diffused together; thus fixed air, diffused among atmospherical air, is visible, till united by solution: as, when disengaged by a mixture of acids and chalk; or by fermentation, as, when a brisk bottle of cowslip wine is uncorked.

4 and 8. The objection you have made under these heads has occurred to another intelligent critick, who says, " that
" a body,

“ a body, when it is made to enter into
“ a chemical union, no longer retains
“ those properties which it had when
“ uncombined.”

This point admits a concise and satisfactory explanation.

In the original MS. of the INQUIRY I had noted the useful distinction between *chemical* mixture and *solution*, according to the judicious discrimination of Dr. CULLEN. *Chemical* mixture destroys or changes the properties of bodies, as in the union of acids and alkalis which compose neutral salts. But bodies united by *solution* retain their peculiar qualities, as, salts dissolved in water, or odours in air. The variolous matter dissolved in air still retains the property of producing the small-pox. The near resemblance between smell and taste, in numerous, indeed in all the substances capable of exciting these senses, proves, beyond all doubt, that
the

the same matter is soluble in water and in air, and retains its original properties in both solutions. Did you never observe that the smell of a chamber, newly white-washed, has a striking resemblance to the taste of lime water?

5. I endeavoured to establish a theory on the observation of facts, but did not make an hypothesis, or supposition, to account for phænomena.

6. I see no reason, as above explained, to think that the variolous miasms constitute a peculiar kind of air.

Some facts have occurred to prove that infectious miasms ascend in the atmosphere: if so, they could not move along the ground so as to do mischief at the distance of fifteen hundred feet.

7. According to my ideas, if a person liable to the small-pox be exposed
to

to air saturated, or nearly saturated with the variolous miasms, he is infected, probably by respiration. In like manner, if a person drink a certain portion of water saturated, or nearly saturated with cathartick salts, or with emetick tartar, he is purged or vomited. The cases to me appear quite analogous to each other.

On the whole, though I have taken the liberty to criticise your letter with great freedom, yet I think it written with much acuteness, accuracy, discernment and ingenuity, and with a truly liberal spirit, highly becoming a philosopher. Your facts, in support of the opinion which has prevailed for many centuries (but which I venture to think erroneous) are more authentick, more accurately detailed and more exactly to the purpose, than any other I have seen or heard of: and, I believe, that they are the best that exist. On these considerations, I gladly accept the
liberty

liberty you give me, of publishing the whole letter, with the substance of this answer. Though your arguments appear inconclusive, yet they are so aptly stated as to be highly honourable to your name; however, I should not publish them if your permission had not been granted. In the introduction to the INQUIRY,* I quoted Dr. FOTHERGILL's anecdote of the wig, which was supposed to convey infection from London to Plymouth, which he related in a conversation at Lea Hall, where, I believe, you were present. His name was suppressed because I disputed his opinion: not that I think the opinion dishonourable for him to maintain. On the contrary, I refer to his ideas as the best that could possibly be obtained. If his notions were erroneous, the whole medical world, it might fairly be concluded, were seduced by a similar deception. His mind was acute, discerning and always exalted far above vulgar preju-

* P. 4.

dices. No man's authority could have had more weight on such a subject. But the conviction of reason cannot be controuled by authority.

I am fully persuaded that Dr. FOTHERGILL entirely changed his idea on this subject. Because, in a future conference, his unreserved sanction, and full approbation were given to the *rules of prevention*, which require no caution in regard to infection from variolous miasms adhering to clothes, &c : and because, in future letters, he expressed his hearty concurrence, and offered his friendly assistance to forward the proposal of exterminating the small-pox from Great Britain. Such a change of opinion, at his time of life, was an uncommon proof of a candid and vigorous mind.

As the people of New England have traced with great assiduity and accuracy the progress of the small-pox, for
Y a long

a long series of years, whenever it has visited them, and as great care has been taken to determine how every person in this distemper was infected, I would earnestly recommend this subject to your attentive and minute investigation. I shall very thankfully receive the accurate lists, kept by clergymen in their own parishes, which you kindly offer to transmit to me. Collect, I entreat you, all the papers which exist among your physicians, clergy and other literary men, and illustrate the cases by all the living evidence which cross examinations can suggest. Note *dates* and *distances*, with all possible exactness. Omit no cause of infection according to the publick creed of the country; though many of these, I will venture to say, will stagger your own faith. Produce every instance on record, "*where wigs, &c. communicated infection after passing through the air for several miles.*" When you have made a complete collection of cases, they will

will highly merit a critical examination, and I will hazard a conjecture that they may be properly classed under the following heads. 1. Probable, 2. improbable, and 3. doubtful. Inquire rationally and impartially how far the supposed causes of infection are adequate or inadequate to the effects attributed to them. Such a discussion is excellently adapted to the discovery of truth, and will lead to conclusions highly interesting to your country and to mankind. Put the doctrine of the INQUIRY to the test of these facts, or rather put these facts to the test of that doctrine.

J. HAYGARTH.

N. B. I am *particularly* desirous to learn how many patients had the small-pox in the houses at the edge of the water in Boston? 2. Were their pustules numerous or confluent? 3. Did they continue in these houses the whole disease, or at what period of it were

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they

DR. WATERHOUSE'S LETTER.

they removed? 4. Can you ascertain the distance at which any and what number of persons liable to the small-pox were placed from these houses in Boston, who yet escaped infection? 5. In what direction did the wind blow, during the infectious state of these patients?

3. *An EXTRACT from the second LETTER of Professor WATERHOUSE, dated Cambridge, October 28, 1788.*

‘About eight months ago, the small-pox was brought into the port of Boston, by a ship from London.* The authority took proper care of the ship,

* In a letter, dated the 15th of October 1787, Dr. Waterhouse says, “I do not believe that there is, at present, a single person infected by the small-pox in all the four New England Governments, that is, not one in about a million of people.” How widely different is such a statement from what could be reported of this distemper, at any time, in Great Britain, or any other part of Europe!

and

and no infection was communicated to any one in Boston. One seaman, who lived in Cambridge, by opening his chest and bestowing some of its contents on his brothers and sisters, gave them the small-pox; and from this individual seaman, I can trace the infection through *Cambridge, Newton, Watertown, Needham, Westown, Brooklyn and Medford*. These towns are in the vicinity of Boston, none of them more than ten miles distant. The Boston markets are daily supplied from these towns, and yet matters are so conducted that there have been but *two people* infected with the small-pox in that city since the above-mentioned London ship arrived!

‘ The authority permitted inoculation in *Watertown, Newtown, Medford and Brooklyn*. Hospitals have been erected in these several towns; but the grand inoculating hospital is at Brooklyn, which is scarcely three miles from

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Boston, and about the same distance from Cambridge.

‘ The buildings for inoculation are situated on a point of land called *Sewell's Point*, which *juts* out into Charles-River. This *point* is not a bleak sandy beach, but ornamented with trees, pleasant walks and green banks, and is a mile and a half from the common road.

‘ The buildings at Sewell's Point are (1.) The *well-houses*. (2.) The *inoculating hospital* capable of containing a hundred and fifty patients. (3.) A *small house* for such as have it very full, or for such as may take it in the natural way; for should any be seized with the natural small-pox within the town of *Brooklyn*, this *licensed* hospital *must* take them in. And (4.) a wash-house or house of purification.

‘ The patients are first taken into the well-house, where the inoculation is performed.

performed. Here they remain until the third day from the first appearance of eruption, when they are removed down to the inoculating hospital, which stands at the distance of half a mile from the well-house, and within a quarter of a mile of the river. While at the well-house, the patients were freely visited by their friends and acquaintance; but, after they are removed into the hospital, all intercourse is forbidden unless the visitor is willing to undergo the operation of the smoke-house. In every stage of the disorder, the inoculated keep, for the most part, in the open air, and exercise themselves with a variety of games and amusements, especially *swinging*. They are allowed a range of perhaps half a mile square.

‘ But those in the well-house, that is, such as have had the matter inserted into their arms, are not permitted to go into the great road.

It is generally three weeks from the period of inoculation until they are dismissed from the hospital. Before they are dismissed, they are washed all over in soap suds, then rubbed with brandy, and lastly washed in vinegar; they put on fresh clothes, and bury those which they wore during their stay in the hospital. But even then they are smoked and fumigated with sulphur in a *smoke-house* which is about twice the size of a common centry-box. This smoke-house has a hole in its side for the patient to put his head out during the operation. Although this seems formidable on paper, yet they submit to it cheerfully, and with no small merriment.

‘ If any person should have the distemper *very full*, so as to be offensive, he is removed into the *small hospital* (3) which is at a few roods distance from the great one.

So

So much for the hospitals, and now for a few miscellaneous observations.

There are perhaps one hundred and fifty under inoculation at present, not one of them *paupers*. They are principally children, perhaps thirty or forty of them children of some of the first people in the common-wealth.—The charge of the whole process is *eight dollars, or thirty-six shillings sterling*, including every expence from the incision to the dismissal. In some places they inoculate for half that sum. You must conceive the whole of this business conducted with a good deal of gayety, where the patient, when ill, is as apt to be pitied, as if he was on a sailing party and sea-sick. Neither do I think the musick and little ceremonies of parade totally useless. An established system of mirth and good humour contributes not a little to their welfare.

You ask whether our sailors are generally inoculated.—I answer, yes, they are

are.—Nearly all who were in the war have had the small-pox, and that principally by inoculation. When *General Washington* inoculated his New England army, there were scarcely men enough free from the disease, or not liable to take it, to keep guard at the different hospitals. (See a *curious fact* in *Colonel Humphries' life of General Putnam*, p. 151.)

‘ We find that in 1752 there were but *one hundred and seventy* liable to the small-pox in the town of Boston, and in the year 1754, when there was a general inoculation in that town, I question whether there was a quarter of that number that did not receive the infection *Viâ Naturæ vel Artis*. In the years 1776, 1777, and 1778 they inoculated pretty generally, through the state. Two days ago, I was at the review of part of the militia of the county of Suffolk, and of *five and twenty hundred men*, I scarcely think there

there were one hundred, above twenty-five years of age, but what had passed through the small-pox by means of inoculation; and of *two thousand* reviewed a week or two before, in the county of Middlesex, there were not a greater proportion of the same age liable to take the disease. I ground my opinion upon the reports of the practitioners living in these counties, and also of the militia officers who commonly know each man and his family. Such is the sociable mode of life in America, and so much does it differ from Europe.

‘ Since the year 1764, the dread of the small-pox has lessened considerably, but since 1778 we meet the disorder with as little fear as any people you can mention; yet we still adhere to our rules and cautions which, from long habit, are become perfectly easy.

‘ You

DR. WATERHOUSE'S LETTER.

‘ You may see, by the following fact, how little the common people dread the small-pox.

‘ A few years since, when the inoculating hospital was on an island in one of our harbours, thirty or forty seamen, from a hundred miles eastward, hired a vessel and came and anchored under this island, and were inoculated on board their vessel and went safely through the disease, notwithstanding they set all regulations as to diet at defiance, and, sailor-like, lived in one continued round of frolick. They nevertheless submitted to a thorough purification; their vessel was fumigated with brimstone, and washed with vinegar as much as if they had the plague on board.

‘ Were I to inoculate a number of sailors, I think I should prefer a good hospital ship to a building on shore, in
order

order, if necessity required, to change the air by sailing.

‘ At Rhode Island the dread of the small-pox is much lessened, although it is still considerable compared with the other states.’

4. EXTRACT *from Professor WATERHOUSE's third LETTER, dated Cambridge, July 21, 1789.*

‘ I still think the carpenters were infected by patients at the distance of one thousand five hundred feet. As there is a bridge since built over the river, the distance mentioned is not guessed at, but accurately measured. You mistake me, if you think I mean that the air was infectious one thousand five hundred feet round the house or houses, making the sphere of infection three thousand feet. On the contrary, I suppose that there is
little

little or no risk of taking the small-pox to the *windward* of such places. The doctrine respecting the facility of communicating infection, as related in my former letter, is, I still think, founded on repeated experiments and happy should I be if you prove that we are mistaken. In this instance, I should rejoice in finding that I was in an error, and as soon as you convince me of it, I will do my utmost to spread your doctrine through America. I think I need not say to you, that I had infinitely rather your doctrine should be found true than mine, as it would tend to lessen one of the greatest calamities that afflicts mankind.

‘ I was exceedingly pleased with the benevolence of your design, and was fearful lest your plan would be marred by an opinion that the sphere of infection was narrower than we from experiment think it is.

I can

‘I can give you some instances which favour your opinion. The one of a lad in the *Andover Academy* (twenty miles from Cambridge) who took the small-pox no one knew how, and the eruptions, which were few, were nearly on the turn before it was known to be the small-pox, and although he was with the rest of the boys in the academy no one took the disease from him.

‘I am led to believe it not unphilosophical to suppose, that the small-pox can be waisted fifteen hundred feet, and more; from this well known fact. We have a small animal among us called a *skunk*, a kind of *pole-cat*, which annoys its enemies by ejecting a most abominable stinking fluid, this may be smelt above two miles, that is, from an island two miles off shore, when the wind sets fair for it. Suppose I should prove *chemically*, in some future letter, that miasmata are easier conveyed in a foggy atmosphere than in a clear air?

From

From the facts related by Dr. DOBSON we find the small-pox is conveyed with more difficulty in a dry one.

‘Accept my thanks also for your other papers. That relative to the *cure of the bite of mad dogs* I shall spread through the continent. I shall send what relates to the charity schools of Chester to the *Columbian Magazine*. You already know, perhaps, that there are free schools in every town in the *Massachusetts*. A free school is essential to the very existence of a town. No new township can be erected without making a provision in lands for the support of a minister and of a free school, where the sons of the rich are educated free as well as the poor, so that there does not exist that order of youth in the *Massachusetts* which your institution is calculated to relieve. Hence it is that the natives are so seldom brought to publick punishment, although the war has injured our morals not a little.

‘I find

‘ I find nothing exceptionable in your letter and remarks ; on the contrary, there is a politeness which characterises the gentleman, scholar, and diligent inquirer after truth. If you should find any thing in any of my expressions which can bear two constructions, please to alter it agreeably to that *disposition* which I wish the publick to believe I bear towards Dr. Haygarth. Besides, there is that difference in our years that will, I hope, always induce me to doubt, when I differ in opinion from one who has had more experience. I am sorry to be obliged to write in such haste ; but these are my sentiments though perhaps carelessly expressed.

‘ Will you be so good as to send me the Cambridge form of a Diploma for M. B. ? We expect to create several, next commencement, and as *our* university was formed on the model of

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your

DR. WATERHOUSE'S LETTER.

your university of Cambridge, we should prefer their form of Diploma to any other.'

5 EXTRACT *from Dr. WATERHOUSE'S*
fourth LETTER, *dated August 5,*
1789.

' Now let me give you an instance or two which seem to favour your opinion rather than our's. Dr. ASPINWALL has inoculated above five hundred persons within these nine months, at his hospitals, which are about two miles from this college.* He informs me in writing, that, during the last year, numbers of people visited his *preparatory houses*, and tarried several hours with his patients, some of whom had had eruptions for two or three days, which were nearly matured ; some had fore arms, with a considerable discharge,

* See Brooklyn Fort, Dr. Gordon's map.

which

which were opened, and shewed to the visitors, many of whom were liable to the small-pox, but no one took the distemper; many children shewed their fore arms in this manner to persons who came to see them, before they were sent into the infected hospital. — He gave another instance. A man was seized with the small-pox in the natural way: his wife attended on him when bled and when he took a vomit, at which period the eruption was fully out, yet none of his family took the disease. Hence it is highly probable that the disorder is not communicated till after maturation.

‘ This gentleman, Dr. ASPINWALL, has read your INQUIRY, and my former letter to you, and yet firmly believes the account of the infection being conveyed from Boston to Charlestown (one thousand five hundred feet) — nay, he seems more sanguine in the opinion than I am, that when several houses

Z 2

were

were highly infected (*as was the case when conveyed across Charles-River*) the contagion may reach as far as you can smell the effluvia of a *skunk*. He tells me also, that it is a fact well known to every plough-boy, that a *skunk* can be smelt much farther in a *dampish foggy evening* than in serene weather and in the day time: I do not wholly join in judgment with him, because the fluid ejected by this odious animal when enraged, exceeds, I believe, in strength all effluvias in nature, or art, insomuch that I do not believe *old Putnam* himself would dare to come to close quarters with one, although they are not bigger than a half-grown domestic cat.

‘ In order to form a just idea of the situation of Boston, Charlestown, &c. please to consult a map in the *Rev. Dr. Gordon's History of the American War*, just published, vol. II. facing the title page; it is a very accurate map;

map; it will give you a good idea of the relative situation of Cambridge, Brooklyn, &c.—You ask whether the wind did not blow from the infected houses on Charles-River into Boston? I answer, the houses stood near the water *under a hill*. You have doubtless heard that Charlestown was destroyed by bombs and carcases thrown from Copse-hill; now Copse-hill overlooks the houses we are speaking of, and is between Hudson's point and the bridge. Besides, during the course of several weeks, there might be one day only in which it was capable of infection:—nay, some unlucky half hour when they were putting the dead into coffins, removing beds, or the like, in which case it is probable the wind blew through the house. Are not sound and smells conveyed with more facility over water than over land?

‘ I could add to the number of cases, where I think the small-pox was taken

Z 3

from

from infected clothes, without ever coming in contact with the sufferers, but am aware that *affirmative* have not the weight of *negative* cases. Yet you may rely on it, we have made many experiments relative to the small-pox, and have not *now* the dread you imagine.

‘ I wish we could converse an half hour together upon chemical principles and chemical language. I have sometimes thought that our chemical creed did not agree, but perhaps it may be only a disagreement in terms.—You say, p. 17, “ It is of importance to ascertain “ the mode of combination between “ the variolous poison and air—I apprehend they *are united by solution.*”—The prosecution of this matter would lead me to the discussion of chemical *combination*, of *solution*, and simple *diffusion*. I rather suppose that the small-pox miasmata are simply diffused in air, and that the dissipation of pestilential

lential effluvia is owing to their specific levity (arising from their aerial exility) and accordingly mentioned to you "that the air near the ceiling of a sick chamber is always most offensive." and you tell me that the observations of Mr. HOWARD confirm my hypothesis. —I do also suppose that animal bodies more especially attract from the air these pestiferous corpuscles, agreeably to the experiments and reasonings of the truly philosophical PRIESTLEY. Are we not therefore to suppose that infection may be conveyed to a greater distance over water than over a field of vegetables? —but more of this hereafter, for I have not time to be very accurate.

‘ In what publication of Dr. Cullen’s is that distinction between mixture and solution to be found? I have never met with it,

6. *EXTRACT from the second ANSWER to Professor WATERHOUSE, dated Chester, October, 1789.*

1. The fact you candidly relate of the boy who had the small-pox in the Andover academy, without infecting his school-fellows, is certainly a case in point. The distemper, when "near the turn," is undoubtedly infectious. You do not mention on what day, from the commencement of the fever, or indeed, whether at all, he was removed out of the house. I take for granted, as the story implies, that several of the other boys were susceptible of infection. A minute and accurate detail of this and other similar facts, whenever they occur, will fairly lead to important conclusions. How many boys, liable to the small-pox, were placed, in the near neighbourhood of the patient, and to what day of the disease did they continue

continue in such a situation without being infected? Can it be accurately ascertained at the distance of how many feet this poison was separated from the boys who escaped the danger? What was the date of this event, and did the distemper spread, at that period, in any and what part of the New England provinces? Such a comparative view of facts would most effectually destroy the hypothetical error, that the small-pox spreads epidemically in some peculiar constitutions of the air more than in others.

The occurrence at Andover academy affords another instructive lesson. The boy was "infected nobody knew how." On this occasion, *you* will agree with me, that the poison, had most probably approached the patient secretly and accidentally, at the usual period before the eruptive fever. In this instance, it confessedly escaped all the watchful and sagacious anxiety of your countrymen.

By

By these dark paths of destruction, the widely and carelessly scattered poison spreads the pestilence through most parts of England: we wot not whence it cometh or whither it goeth. And, I am much inclined to surmise, that it eludes your search, even in America, much oftener than you are willing to believe. By placing an undue portion of confidence upon an erroneous hypothesis, you must frequently drop your pursuit of the true cause of infection, before its discovery. The patients who were supposed to have caught the contagion from Dr. Brattle's and Mr. Anthony's wigs, as well as the carpenters of Charlestown, to whom it was supposed to have been waisted fifteen hundred feet across the river, from Boston, and other like marvellous cases, might, and probably had previously received the poison "nobody knew how." Such cases as this, where no cause of infection, true or false, can be *suspected*, are well adapted to confute the hypothetical

thetical prejudices, which, I hope, you will assist me to combat. They clearly prove the ambiguity of *affirmative* evidence.

A fact, in regard to another infectious distemper, extremely similar to your's, fell under my own observation: the inference deducible from both strongly confirms the general doctrine. In April 1779, Master Plumbe, the son of a gentleman of fortune near Liverpool, was attacked, in a dangerous degree, with a scarlet fever and sore throat, in the house of his schoolmaster, the Rev. Mr. Vanbrugh, at Chester. There were at the time thirty-seven young gentlemen, boarders in the family, most of whom it is highly probable were disposed to receive this dangerous contagion. My patient's chamber was situated in the middle of the house, at the landing of the first pair of stairs: all the scholars went close past his door several times a day.

a day. At this season, Winchester and several other large schools in England sent home and dispersed their scholars, on account of this distemper which had alarmingly spread among them. Whether this measure, with all its inconveniencies, was not advisable, became a very serious question. The numerous facts which I had then collected to prove that the variolous infection, though probably the most virulent we are acquainted with in this climate, exerted its baneful influence at but a small distance only from the poison, encouraged me to hope that the contagion of a scarlet fever was incapable of producing more extensive mischief. *The rules of prevention* were placed on the door of the patient's chamber, and rigid attention to their faithful observance was required. The event fully justified my hopes. Though all the thirty-seven scholars remained in the same house and family during the whole disease, yet not one of them was infected.

I do

I do not recollect any observations recorded by authors to determine what proportion of mankind are liable to the attack of the scarlet fever. In October 1778, out of forty young ladies at a boarding school in Chester, all but four had the distemper, twelve very severely and two most dangerously. This comparative statement of facts, which happened in the two schools, shews, beyond all reasonable doubt, to what a little distance from the poison the infectious miasms extend, and that the *rules of prevention* are, in this respect, fully adequate to their purpose.

2. The persons who visited Dr. Aspinwall's hospital probably avoided infection, because they might not *nearly approach* "the patients with eruptions" out for two or three days and nearly "matured," or "the sores with a considerable discharge, which were opened." The pustules of the former and the sores of the latter appear to have been in an infectious state.

As

As you candidly allow the superior force of *negative*, in comparison of *affirmative* evidence, to decide this question, I cannot any longer doubt your becoming a firm and faithful proselyte to the new doctrine. In considering this question, the reader is apt erroneously to associate two ideas, owing to the equivocal signification of words. *Affirmative* proof may be thought to mean *positive* proof, which has a double signification: it not only denotes an affirmative, but farther, a complete and undoubted proof. By this accidental association of ideas, the reader's understanding, without great care and calm consideration, is inadvertently betrayed into error. One of the most ingenious of my correspondents actually made this mistake.—In like manner, the expression of *negative* proof, without due attention, is thought to imply an imperfect, and incomplete proof. We all know that a certain fact may, or may not, have happened.

Either

Either of these events may be capable of proof. In many cases, the affirmative proof is more easily established; in others the negative; and particularly so on the points we have discussed, as, I am glad to find, has been satisfactorily argued to your own conviction, and to that of many other impartial readers of sound understanding, and superior abilities.

I shall touch, very briefly, on some theoretical points. The notion of solution, which I have adopted, is the simplest of all chemical principles, and is explained in all authors who treat on this subject. It is hardly necessary to refer to any. In the accurate and elegant essays of bishop WATSON, he says "a perfect transparency of the fluid, "in which any body is dissolved, is "esteemed the most unequivocal mark "of its solution."* The distinction

* See vol. III. p. 76. and vol. I. p. 78—82.

made

made by Dr. CULLEN in his chemical lectures, though ingenious and instructive, may not perhaps be published in any book I can refer you to, and not being essential to the question, may be omitted.

It is true that odours are most perceptible in an *evening*; but, I believe, not more so in a foggy day or evening, than when there is no fog, except what arises with the evening dew.

Your ingenious friend, who infers that the small-pox might be caught through the space of two miles from the poison, because the smell of a skunk has been so extensively perceived, might conclude by parity of reason, that the odour of a variolous pustule or of a rose must be sensibly distinguished, at the distance of two miles.

You maintain, that variolous miasms can infect children to the distance
of.

of fifteen hundred feet in the direction toward which they are blown by the wind.

You suppose, I take for granted, that they would infect every person liable to the distemper in that line of direction. And, as the wind moves to various points, both in your climate and our's, in a week, a day or even an hour, this pestilential line as a radius, will sweep over an area of considerable dimensions,—over a large portion, if not over the whole superficies of the circle.

An accurate detail of facts affords the most satisfactory illustration of an obscure cause. As the patients in Boston were situated under a hill (Cope's) the miasms must have *ascended* to a considerable height before the current of wind could have blown them across the river ; and, consequently,

A a they

they must have been mixed with a large mass of fresh air, before they could, by some unknown quality, have *descended* again among the carpenters. Various objections on this and other grounds might be suggested against your hypothesis, but they seem to be wholly superfluous.

It may be proper to mention, that Mr. HOWARD's instructive remark, communicated in my former, and referred to in your last letter, has since been published in his work on lazarettos, prisons, &c.* He briefly observes that, "last year, seven of the
" felons (at Stafford) died in their dun-
" geon, of the goal fever: and the
" free ward, or county chamber, being
" *directly over* it, nine out of thirteen
" of the poor debtors died."

In conversation, Mr. HOWARD explained to me this event, more fully.

* P 173.

He described the floor which divided the felons from the debtors, to be full of fissures, so as to admit a free perflation of pestilential effluvia from the lower to the upper chamber.

To determine whether the infectious vapour of fevers can ascend to the cieling of a room, might require much patient and sagacious observation. But I have long thought that the air immediately arising from the body of such a patient must be much more pestilential, than any other portion of the atmosphere of a sick chamber, except what had been expired from his lungs. It may be difficult to ascertain whether, in the same temperature, it is specifically lighter than common air : but it must be *warmer*, and, for that reason, will constantly *ascend*. On this consideration, a medical visitor ought not to respire the air perpendicularly *over* a typhous patient, which contains, it is probable, a large proportion of infectious vapour.

A a 2

Accept

Accept my best thanks for the favour of Dr. Boylston's rare tract on inoculation; of General Putnam's Life; of the Inaugural Dissertations for your degree of M. B.; of the Questions and Theses proposed by your Bachelors and Masters of Arts.

Your questions in mathematicks, metaphysicks, natural philosophy, ethicks, and, I believe, most of the rest, are plainly borrowed from what are annually agitated in our university.

As you mention, that *your* Cambridge proposes *our's* as a pattern for imitation; I will add a few remarks on this subject. Your publick disputations are said to be confined to the day of commencement for the entertainment of the ladies and gentlemen assembled on that occasion.

The disputations of our Sophs, in the publick schools, during the year previous

vious to their graduation, are serious, improving, and arduous exercises. With some modifications, they highly merit your adoption. Each student, during the year which precedes his degree of A. B. is required to keep two respondencies and five opponencies. The Respondent proposes one question in morality or history, another in natural philosophy, and a third in mathematics. On one of them he composes and reads a Latin Thesis. Three Sophs, selected from the different colleges, of nearly equal abilities with himself, are appointed his Opponents; the first produces eight; the second five, and the third three arguments against the proposed questions.—To each of these, the Respondent is required to give an extemporary answer. Two Masters of Arts, eminent for their learning and abilities, are annually appointed MODERATORS; of whom, one constantly presides over these disputations, and ascertains with accuracy and fidelity the

various merits of the young gentlemen, from the lowest to the highest character. He dismisses them with a publick declaration of his opinion of the abilities which they have exerted in the debate : and generally in the following words : “ *descendas ;* ” “ *fatis fecisti ;* ” “ *benè & fatis ;* ” “ *perquam benè et fatis ;* ” “ *optimè et fatis disputasti.* ” The Moderator’s sentence is always entered into a book and taken into consideration in determining the honours to which the candidates are entitled on admission to the Bachelor’s degree. The disputation is in Latin, and the objections are proposed and answered in logical form : it is constantly frequented by the members of the university, both students and graduates, in greater or fewer numbers according to the reputation of the disputants. It is always conducted with gravity and decorum ; sometimes with interesting animation, with a display of acuteness, discernment, deep knowledge and powers of argumentation that
would

would do honour to any philosophical assembly.

Another regulation of *our* Cambridge highly merits *your* imitation. At the Bachelor's commencement, about thirty-six of the Sophs are distinguished by honours, in the exact order of their merit. The first twelve are discriminated by the whimsical appellation of *Wranglers*, a great object of ambition. In academical language, each year is distinguished by the name of the first Wrangler, among contemporary Cantabs. Thus the justly celebrated PALEY was the senior Wrangler of my year, in 1763. The second twelve are called *senior Optimes*, and the third twelve *junior Optimes*.

The competition for these honours continues for four days, and for six hours each day. The candidates are examined in mathematicks, as, Euclid, plain and spherical trigonometry, alge-

bra, and fluxions; in natural philosophy, as, mechanicks, hydrostaticks, opticks, and astronomy, including NEWTON's *Principia*: in metaphysicks, ethicks, and the foundation of natural and revealed religion, as, LOCKE's Essay on the Human Understanding, RUTHERFORTH's Institutes, PALEY's Moral and Political Philosophy, and BUTLER's Analogy.

On the third day, the publick part of the examination is solely confined to moral philosophy: on that day, at twelve o'clock, a list appears, which states the relative merit of the competitors. It lies open for general inspection. If any one think himself aggrieved, he may complain to the Father of his college; and one or more disinterested men are appointed to determine the relative merit of the challenger and the person challenged.

The four publick examiners are the MODERATORS of the last and present years:

years : by whose judgment, the honours are chiefly ascertained. All Masters of Arts have a right to examine, and they are appealed to on all contests of relative merit.

An extensive knowledge of the Greek and Roman classics, deservedly obtains great encouragement, and attention from the lectures, honours and emoluments of all the colleges, and from various university regulations ; but, unfortunately, it is not included in the examinations for a degree. The branches of science, which are made the subjects of competition, it is manifest, are highly important, but some not less meritorious, are omitted. By including languages, and perhaps three or four other subjects of literature and science, which a by-stander might think easily accomplished, the examinations for a degree would possess uncommon and approach complete excellence. The wit of man cannot devise
more

more powerful incentives to emulation than this mode of graduation. I purpose to send you *Beverley's* account of the *forms* of taking degrees, &c. at Cambridge, which will best communicate the intelligence you desire; but warmly recommend to your adoption the impartial distribution of honours, as greatly more important than the ceremonies of our university.

The examinations, twice a year, at St. John's, instituted by my highly respected tutor, the late Dr. POWELL (of which I intend to inclose a copy) seem to merit your adoption, and, in that college, supply the defects above stated.

I recommend some of our regulations, but disapprove others. You inquire what is our mode of taking medical degrees. To commence M. D. you, like us, require *personal* attendance, several years after having become M. B. Now a physician, in full practice,

tice, can never have leisure, nor can with propriety desert his station, during the time required, for such a graduation. Above twenty years ago, I had an inclination to revisit Cambridge to be created M. D. But at that period, and ever since, no possible opportunity has occurred, when, with propriety, I could have spared time for such a purpose. I should have been very justly blamed for neglecting a much more important duty.

As America is larger than England, the inconvenience of personal attendance to take a second degree, at your university, must be proportionally greater. However, an additional degree ought certainly to be conferred as a mark of superior merit, and some proof of improvement should be exhibited. Medical Dissertations, containing *new* and *useful* remarks, not copied from books, but suggested by experiments or by clinical observations, might, in certain circumstances

circumstances at least, be accepted as a test of advancement in medical knowledge.

VII. EXTRACT of a LETTER from
THOMAS HENRY, F.R.S. dated
Manchester, October 20, 1789.

* Though you may remember, I, on another occasion, could not fully subscribe to the whole of your chemical theory, yet I am satisfied that your conclusions are just; the difference between us being such, as not to affect the inferences you have deduced.* I am

* Mr. HENRY refers to a former letter, in which, at my request, he wrote, with great acuteness of argument, a criticism on the chemical theory that *the variolous miasms are dissolved in air*; which first suggested the observation of all the most important facts related in the INQUIRY. But it need not be here inserted, as the reader will probably think that too many theoretical remarks have been already submitted to his consideration, and because I can honestly repeat, what was said at the conclusion

am well convinced that variolous miasms cannot long adhere to clothes, when exposed to the action of the air. Whether these miasms be in a state of solution with the air or not, they may certainly insinuate themselves, either *per se*, or combined with air, into the clothes of a person who enters the sick room; but from their volatile nature it does not seem probable that they should retain their situation for any considerable period. In some circumstances indeed, this period may be prolonged, as e. g. in very cold weather, the evaporation, or, according to your theory, the solution may be impeded, and thus the miasms may remain attached to the clothes, till a warmer atmosphere sets them in motion. But that a man, who has been in a small-

conclusion of my reply to Mr. Henry's letter. "On the whole, your very ingenious criticisms (like those of our excellent friend Dr. Aikin) on the subject of this theory, have increased mine, and, if I am not mistaken, have not diminished your confidence in its accuracy and truth."

pox

pox chamber, without coming into contact with variolous *matter*, can convey the infection several miles, through the open air, and communicate it to another person who converses with him at a distance, is beyond the limits of my creed. Neither can I possibly believe that variolous miasms, mixed with atmospheric air, could be conveyed to the distance mentioned by Dr. WATERHOUSE, retaining their noxious qualities so as to produce the small-pox in a person liable to infection. Your arguments, with respect to the effects of the immense degree of dilution which must take place before they could make such a progress, are, in my opinion, unanswerable.

‘ But experience seems strongly in your favour; nor can a few detached facts, such as have been adduced by your ingenious American correspondent, weigh against the mass of evidence you have brought forward. The
gentleman

gentleman who is supposed to have carried the infection, so many miles, to the lady who conversed with him through the window, might have laid down his hat so as to touch some varicellous matter; or the lady might, without being conscious of it, have been near some convalescent, or been otherwise exposed to the influence of contagion. I have frequently seen beggars place children, scarcely recovered from the small-pox, and with the dirty clothes which they had worn in the disease, on the steps leading to houses where there have been persons liable to the disease. In this way, they might receive the infection without any suspicion of the means by which it was conveyed.

Perhaps there is not a more common mode of communicating infection, than in garments received from tailors, mantua-makers, &c; and families thus become infected, when they are con-
fining

fining their children within their houses to avoid the disease. The following anecdote was related to me by a lady who was a witness of the fact. Many years ago, a bride made her first appearance at an assembly in Liverpool. The gentleman who danced with her, on being asked, the next morning, how he liked his partner, declared she was a very agreeable woman, but that she smelled so strongly of the small-pox, that it was with difficulty he could bear to stand near her. He was much laughed at for, what appeared, so whimsical an idea; and the impossibility of the fact was asserted, as she never had had the disease. In about a week after, however, she sickened and died of it; and it appeared, on inquiry, that her clothes had been made at a mantua-maker's in London, where several of the family at that time had the small-pox.

‘ For my own part, I am satisfied, that with proper precaution not to carry

carry off any actual matter; the danger of conducting variolous infection to any great distance from the chamber of the patient is very inconsiderable. I have been in the habit of visiting persons in the small-pox, for near forty years, and I do not recollect a single instance, where I could charge myself with having thus carried infection in my clothes, so as to infect a person who had not had the disease. Though I must very frequently, in the course of my practice, have been under the necessity of visiting such persons, within a very short time, perhaps within fifteen or twenty minutes, after having been exposed to variolous effluvia.

‘ In large and populous places, such as Manchester, the small-pox almost always exists in some parts of the town. I have known it strongly epidemic in one part, without any appearance of it in others. And this, notwithstanding the variolous patients have resided in

B b houses,

houses, adjacent to the publick ways, where numbers of children liable to the disease, must necessarily pass. It is, indeed, the case at this present time. The disease is prevalent, and fatal in the outskirts, but very rarely occurs in the interior parts of the town: whereas, were the air capable of conveying the infection, in the manner supposed, no district, to which the wind could direct the mass of air surrounding an infected house, could escape a participation of the poison. But how frequently must every practitioner recollect the small-pox to have afflicted the family of one house, in a neighbourhood containing numerous families, several of the members of which were liable to infection, and yet have not received it. Nor is this to be wondered at, considering the immense quantity of air with which the effluvia are united, and the continual streams of fresh air which are continually arriving,

iving, and removing the former masses, while but slightly impregnated.'

VIII. I. EXTRACT of a LETTER
from JOHN CLARK, M. D. Fellow
of the Royal College of Physicians at
Edinburgh, dated Newcastle, March
15, 1791.

' Agreeably to my promise, I proceed to give answers to your queries: and in doing this, I shall sometimes offer a detail, and at other times the result of personal experience. But although I have, for many years past, carefully attended to the propagation of the small-pox, and other contagious diseases, yet, I fear, what I have to advance will not answer your expectations; at least, I am certain, must fall infinitely short of the great magnitude

of the object, in which you have been so long, and so laudably engaged. This, however, shall not prevent me from submitting to your candour, such observations, as have occurred; being certain you will make proper allowances for the oversights, and mistakes, which must ever attend so intricate a subject.

Answer to Query I.

‘As far as I have had opportunity of making observations, it seems reasonable to conclude, that during the period of eruption, even when the intercourse is as close as possible, a patient, under the small-pox, does not communicate infection to another liable to the disease. If this point could be ascertained beyond a doubt, it would be of much importance: for nothing has been found more conducive to render the disease mild, than free exposure
to

to the open air, till the eruption be fully completed.

‘ The following facts I beg leave to offer in support of the above opinion. They are, indeed, too few ; however, I do not think their force can be obviated by the hypothetical fancy, that inoculation anticipates the natural small-pox.

‘ 1. James Cowel, aged two years, had lain in the same room with his brother, who wrestled through the confluent small-pox, and was very frequently in the arms of his mother, who was the only nurse of both the children. On the first day I visited the family, I recommended inoculation ; but not meeting with the concurrence of the father, it was delayed to the third of February 1779, being the eleventh day from the commencement of the eruption in his brother. On the fifth day from inoculation,

B b 3

lation, a large variolous pustule was observed upon his chin, with a small lymphatic swelling under it. In the evening he became feverish. On the following morning, the inflammation round the incision of his arm was considerable. On the seventh day, from inoculation, there were several pustules and an inflamed disk observed round the pock on his chin; and the lymphatic tumour now resembled a swollen gland. Several distinct pustules then also appeared on his breast and sides. On the eighth day, the fever totally subsided; and he had a very trifling number of eruptions: one pustule filled upon the place of incision; no other vestige of the inoculation remained after the eruption was completed.

‘ Early in the attendance on the brother, his mother had several pustules on her breast; and as James Cowell often lay with his head on her breast, there

there cannot remain a doubt, but that he received the disease, by inoculation, from his mother.

‘ 2. John Jackson, aged five years, was inoculated on the ninth of June 1779; being the fourth day after the appearance of the pustules of his sister Jane Jackson, aged nine. She laboured under a numerous eruption of the natural small-pox; which was ushered in by a very smart fever and diarrhœa. They, from the beginning had lodged in the same small room; and the bed being within three or four feet of the door, he could not possibly go out or return, without being exposed to the effluvia, if any thing noxious at this time issued from his sister. On the eleventh of June, the inoculated incisions began to inflame: on the seventeenth he sickened; and the fever continued smart for two days, notwithstanding the use of an opening medicine, and free exposure to the air. He

B b 4

had

had a considerable number of pustules, but his disease was so mild that he went abroad daily.

‘ 3. Ann Jackson, aged two years, who had the same intercourse with her sister, as the brother, whose case is above narrated, was inoculated, with the same matter, on the ninth of June, that is, on the same day with her brother. The infection not seeming to have taken place in the arms, on the eleventh of the same month, some fresh fluid matter was again inserted. On the thirteenth the inflammation began to proceed rapidly: the eruptive fever was so slight as scarcely to be observed, and a few very distinct and perfect pustules succeeded.

‘ 4. and 5. John Ritchie, aged two years, and William Ritchie, aged six months, lay in a small confined room, with their sister Margaret Ritchie, who died on the twelfth day of the confluent

ent small-pox. Mr. Anderson, by my desire, inoculated them both on the fifteenth of June 1779, being the fourth of the small-pox, reckoning from the first appearance of the pustules in their brother. John Ritchie was a weakly ricketty child, for whom the bark was immediately prescribed: William Ritchie was healthy. They were both ordered to pass most of their time in the open air, during the day; but at night they were obliged to lie in a corner of the same room with their sister, under the natural distemper. The inflammation on the inoculated parts proceeded very rapidly; they had very little fever during the eruption, and only about thirty pustules each.

‘ 6. Grace Storry, aged one year, was inoculated on the sixth of September 1779, being the fifth day from the eruption of a numerous crop of the distinct small-pox on her sister, aged four years, with whom she constantly associated.

associated. On the tenth day from inoculation, her body was suffused with a red rash, interspersed here and there with eruptions like the small-pox. The eruptive fever was moderate, and after the rash disappeared, she had only thirty pustules of the small-pox.

‘ 7, 8, 9, and 10. Elizabeth, Mary, John, and George Hutchison, aged two, five, seven and nine years, were inoculated by Mr. Anderson on the seventh of October 1779, with fluid matter taken from their sister, a patient of mine upon the Dispensary, on the fifth day of the confluent small-pox. They all lived in the same small room. The eruptive fever in all was moderate, and disappeared on the seventeenth of October. A few pustules only appeared on the face and arms of each patient.

‘ 11. Jane Stewart, aged seventeen months was inoculated by my desire on the
the

the eleventh of October 1779. She lived in the same room with her brother; who was, at the time she was inoculated, advanced to the fourth day of the eruption of the confluent small-pox, and who died on the twenty-first of October. The infection went on in the usual way in the arm, the eruptive fever happened at the proper time, and was succeeded by a few pustules.

‘ 12. Another child where the personal intercourse was not constant, but who lived in the room above Jane Stewart, was inoculated on the sixth day from the eruption of the pustules in the child who died; the small-pox succeeded at the usual period from inoculation; and he had very few pustules. It is proper to observe that this patient was two years old, that two days before the eruption of the small-pox a moderate rash exactly like the measles appeared, which stood the usual time, and which I believe really was the measles.

‘ Two

‘Two other children, who lay in the same room with the brother, who had the confluent small-pox, were inoculated on the seventh of October 1779, being the fifteenth day of their brother's eruption. But as the connection had been so intimate, and as one fevered on the fourth day after inoculation, and the pustules appeared on the fifth day, and as the other was only one day later, I shall not say, even although both the diseases were exceedingly mild, that they had not taken the natural small-pox. It must, however, be observed that the inflammation on the arms of each proceeded rapidly: and that a weakly infant of the same family who was not inoculated, sickened at the same time, and sunk under a load of small-pox.

‘I have had no farther opportunity of trying the effects of inoculation in similar circumstances: but, till your plan of exterminating the small-pox
be

be completely carried into execution, I am persuaded that many lives may be saved, by inoculating children, who have been daily exposed to the natural small-pox, during the four or five first days of the eruption.

Answer to Query II.

'The rules of prevention are not only easy to be reduced to practice, but contain every necessary precaution. However, from experience I know, that shop-keepers of almost all denominations, not only neglect every rule of prevention, but, lest their trade should suffer, conceal, as much as possible, every instance of the natural small-pox, which occurs in their families. In large towns, where the disease is constantly prevalent, it would seem to be a necessary precaution, for every family, liable to the natural small-pox, to wash or otherwise purify all kinds of goods, from retail shops, before they
be

be suffered to approach any person liable to the distemper. An additional rule of this kind, as applying to those who are most likely to suffer by the neglect, appears to me to be necessary in your plan for exterminating the small-pox.

Answer to Query III.

‘ General inoculations of the poor, have been attempted, in Newcastle every spring and autumn, since the year 1785. But as recent matter was not generally used, and consequently a great many failures happened,* I can draw no conclusion towards the solution of this query, till I came to the inoculations of the spring and autumn of 1790.

‘ In this year, with a very few exceptions, the inoculation was performed by Mr. Anderson, surgeon,

* See one instance hinted at above, p. 303.

and

and Mr. Wilkie, apothecary to the Dispensary. The matter used, in every case, was recent; the operation performed in both arms, and the failures happened in their patients.

‘ Of three hundred and twenty-eight patients inoculated, this year, at the Dispensary, three hundred and eleven had an eruption of pustules; in seventeen the infection had no effect upon the system; but of this last number, it is proper to observe, that five children, from four to nine years old, had been frequently exposed to the contagion of the small-pox, and, therefore, in all probability were not susceptible of the disease. Deducting these five children, the number inoculated this year will be three hundred and twenty-three, and the failures only twelve.

‘ Inflammation or suppuration in the arm, in consequence of inoculation, has been considered by many, as a sufficient

ficient security against any future attack of the small-pox. But this in one instance, within my knowledge, proved a fatal mistake. During the the first inoculation, performed in Newcastle, two children had inflammation, and large suppuration, without any other effect upon the system: they were both inoculated a second time soon after; and each had an eruption of several pustules upon the body. Other instances have also occurred, to prove that inflammation and suppuration, without fever and consequent eruption, ought never to be depended upon.

Answer to Query IV.

‘No instance has occurred to me, where I could have the least suspicion that the small-pox, was communicated to any person, by another carrying only infectious miasms out of a small-pox chamber. I have also applied to many medical friends for an answer to this query,

query, all of whom have replied in the negative, except one gentleman. He inoculated his own child ; on the sixth day after the operation, it became feverish, and one variolous pustule was observed upon its back. A plentiful, though distinct eruption, succeeded. As the eruption happened at an earlier period, than had before occurred in his practice, he cannot be persuaded, but that it had previously taken the natural disease. He attended some patients in the small-pox ; but was extremely careful, that he brought home no infectious matter on his clothes. I visited this child ; as the infection went on well in the arm, and as the eruption did not take place sooner, than what happens frequently from the insertion of recent variolous matter, I never had a doubt that the distemper arose merely from inoculation.

C c

Answer

Answer to Query V.

‘ In my attendance upon patients in the small-pox, I have been always careful to enjoin free ventilation of the chamber; to keep at so great a distance as not to suffer my clothes to touch the patient; and, after feeling the pulse, to wash my hands. With these precautions, I have frequently soon after visited other children liable to the disease, and am certain, that in eighteen years practice, I never, in one instance, communicated the contagion.

‘ As there is the strictest analogy between the small-pox, and scarlet fever attended with ulcerated sore throat; both being contagious diseases, and only seizing the same person once, I must also mention, that, after using similar precautions, I have, on the same day, visited many patients, in the most malignant stages of the latter distemper,

temper, and have, soon after, had intercourse with other children liable to the disease, but never communicated the contagion ; not even to children in my own family.

‘ But, although I can give no instance of the small-pox having been communicated merely by miasmata, yet, were I to judge from other contagions, I should think it extremely probable, that when cleanliness and ventilation are neglected, the air of a chamber may be so contaminated, or supersaturated, as to render clothes infectious, even after they have been, for a little time, exposed to the open air. In proof of this opinion, I shall adduce the following facts.

‘ A gentleman, in August 1785, was sent for, in a great hurry, to visit a lady, at a boarding school. Upon entering the chamber, he found a most offensive feter ; which made him im-

mediately open a window ; and he stood between it and the patient's bed to make the necessary inquiries. He found that she had laboured under a dysentery for nine days ; and that, for more than twelve hours, she had passed putrid, bloody and involuntary stools. He also found, that she had laboured under hiccup ; that her bowels were inflated ; her countenance pale, her eyes sunk ; her skin clammy, and her pulse so frequent, and feeble, as not to be reckoned. Nothing was prescribed. He immediately quitted the room, and the patient died in less than an hour. The distance from the patient's house to his own home did not exceed two hundred and fifty yards. Upon his arrival at home, he found his only child, a healthy sprightly boy of fifteen months old, at play, in a small parlour. He inadvertently took the child upon his knee ; in two days he fevered ; and, on the following day, he had several bloody mucous evacuations, attended, through

through the whole course of the disease, with so violent a tenesmus, that a glyster could never be retained a moment. Several days elapsed, in spite of every means, before a feculent stool could be procured: on the eighth from the commencement of the dysenterick symptoms, convulsions supervened, and put an end to the severe sufferings of the child. Another young lady, who visited at the same house with the former patients, where the dysentery prevailed, also took the distemper, and recovered: but the rest of the young ladies in the same school, by using proper precautions, escaped this contagion.

* A low fever, exactly resembling the jail distemper, was generated in the habitations of the poor, and, for some months past, has been prevalent in Newcastle. A young gentleman, under my care, is now only in a convalescent state. It was the eleventh day when I first visited him, and the dis-

temper not being thought contagious, his chamber was kept close. From the time I first saw him, great attention was paid to cleanliness, and ventilation : but he lay in a camp bed, and his eyes were so impatient of the light, that, though almost insensible in every other respect, he would not suffer the curtains to be kept open. In a few days after my attendance, two persons, who had been much in the room, sickened of the same disease : and a young girl, in the same house, who was never in his bed chamber, but had intercourse with his attendants, also took the disease. A nurse, who was engaged three weeks after the commencement of his illness, is now also extremely reduced by the fever.

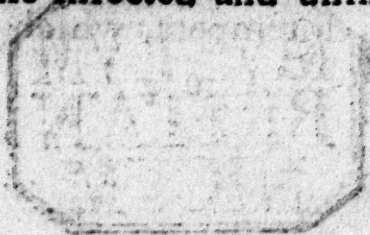
‘ Mr. Rayne, his surgeon, who attended him with the utmost humanity, several times every day ; who was sent for often to administer opiates after loose stools ; and who dressed his blister ;

ter; although he was seized with no symptom of fever himself, carried home the contagion to his wife; a lady of a healthy and vigorous constitution. For ten days before the attack, she had never been out of her own house; which was in the same street, at less than the distance of two hundred and fifty yards, from the place where the young gentleman lodged. She was seized at first with vomiting, constriction above the eye-brows, feverish fits, and severe head-ach. When I visited her on the third day, her skin was a little hot, her eyes listless, and impatient of the light; her pulse weak and frequent; and she complained of great languor; and was unable to sit in an erect posture. By the liberal use of the bark and cordials, her fever was nearly subdued in eight days; but even at the end of fourteen days she complained of extreme debility.

‘ These being the only instances, in contagious distempers, which have giv-

en me suspicion, that, in certain circumstances, the effluvia impregnating the clothes of visitors in a sick chamber, may be so concentrated, as not to be rendered innocent by slight dilution in the open atmosphere, I thought it right to mention them. But, at the same time, they in no degree shew the inefficacy of your rules of prevention; for if they be strictly complied with, the air in the chamber of patients under the small-pox, and other contagious diseases, will never be so contaminated, as to communicate an infectious influence to the clothes of visitors.

‘ In the inoculated small-pox, even when some of the patients have had a numerous eruption, I could adduce many instances, of other children living in adjoining houses, who escaped the infection, by not being suffered to come near the inoculated—even although intercourse had daily taken place betwixt the infected and uninfected families.



milies. But as, at this time, I can only recollect the circumstances from memory, I consider any farther detail to be perfectly superfluous; especially as you have offered a number of well attested facts, sufficient to convince any medical and philosophical inquirer, not only of the limited state of variolous contagion; but also that clothes, exposed to miasms, are at least very rarely contagious.

‘ In your INQUIRY, page thirty-two, on the authority of Baron DIMSDALE, the eruptive fever in the improved method of inoculation, is said to commence on some day, from the sixth to the fourteenth, both inclusive; and at page forty-four you observe, “ that “ you have received information of “ several well authenticated cases where “ inoculated patients have not become “ feverish till several days after the “ fourteenth, but that you believe they “ were not inoculated with *fluid matter*.”



‘ I shall

‘ I shall subjoin the following facts, which will evince that the eruptive fever may often be suspended beyond the fourteenth day, even when fluid matter is used.

‘ My own child, four months old, was inoculated with recent matter, taken from the pustule of a child on the eighth day of the distinct small-pox, which was immediately inserted in three places, at a small distance from each other, on his arm. The incisions inflamed rapidly, and after the tenth day, from the appearance in the arm, I hourly expected some fever, or eruption of pustules. On the seventeenth day from inoculation, he began to complain; the fever soon ran high, and continued without the remissions which usually happen in a mild disease. On the twentieth the pocks appeared, and continued to do so for at least three days. He had a numerous, though distinct eruption, attended with no alarming consequence. The arm afterwards

wards suppurated largely, and was several weeks in healing.

‘ In the inoculations at the Dispensary, in 1790, where fresh fluid matter was used, one child did not sicken till the sixteenth, and another child till the seventeenth day after the operation. The eruption was very mild in both patients.

‘ Hannah Thomson, aged two years, was inoculated at the Dispensary on the thirteenth of April 1790, with fluid matter: on the twentieth and twenty-first she had a rash over her body and appeared a little feverish. The incisions still kept inflaming and spreading; on the fifth and sixth of May she fevered; on the seventh of May, being the twenty-fifth inclusive from inoculation, the eruption commenced; and she had in all about two hundred distinct pustules.

Physicians and surgeons from the rude practice of inoculators, in suffering

ing their patients to remain for some time with others under the disease, before they insert the variolous matter; and the event of such procedure having never been by them recorded, as attended with any inconvenience, have hastily concluded, that inoculation anticipates the natural contagion: or, in other words, that the distance betwixt the infection and the eruptive fever is shorter in the inoculated than in the natural small-pox. But my observation warrants no such conclusion. If no fatal accident has happened to inoculators from this practice, I should suspect, that their patients liable to the small-pox, have either only associated with the inoculated during the first days of the eruption, when infection is little to be dreaded; or that, in their well aired houses, the effluvia of their inoculated patients have been so much diluted, according to your theory, as to communicate nothing noxious to the air they inspired.

The

The following table will exhibit, at one view, the period from inoculation to the commencement of the eruptive fever, in the patients inoculated at the Newcastle Dispensary in spring and autumn 1790.

Day after Inoculation.	Number of Patients. NEWCASTLE.	At the Small-Pox Hospital, LONDON,* in 1791.
On the 5th	2	4
6th	6	40
7th	17	141
8th	77	232
9th	96	50
10th	73	30
11th	22	2
12th	8	0
13th	3	0
14th	3	1
16th	1	0
17th	1	0
23d	1	0
No perceptible fever	1	0
Remained uninfected.	12	10
	323	510

* This intelligence I have just received from Dr. WOODVILLE, who was appointed physician of the Hospital, a few months ago. From this and other instructive observations on the subject of inoculation, which he has already communicated to me, I entertain sanguine hopes, that, in this station, his time and abilities will be employed in a manner that will be highly honourable to himself, and beneficial to mankind.

Having

Having made all the observations, which have occurred to me relative to variolous contagion, so far as they would seem applicable to your Queries, permit me to conclude with the following reflections.

‘ In the execution of your benevolent project, you have thrown much light upon contagion in general, you have detected its confined sphere of action, you have removed many prejudices which were attached to the subject, and by the exertions of your society at Chester, you have evinced the practicability of preventing the natural small-pox; and, therefore, should you not have the happiness to see this pestilence soon exterminated from your native island, yet still your labours have been of the utmost advantage to society.

‘ But although your plan of prevention is easily practicable, and would exterminate the small-pox from any place,

place, where the inhabitants by unanimous consent strictly observed the regulations; yet, in populous towns, where the disease is constantly present, and where the greater part of the inhabitants consider it to be a natural evil, the execution of the project will be attended with extreme difficulty. In our attempts towards a general inoculation in Newcastle, in vain has every endeavour proved to convince the poor of the possibility of preventing the small-pox: nay even a great majority are so ignorant as to believe it to be not at all contagious. With such people, all arguments are fruitless; and unless your plan meet with the concurrence, and assistance of the LEGISLATURE, and be carried into execution by the vigilance of the civil magistrate, I despair of seeing this loathsome and depopulating disease expelled from the British realms.'

2. EXTRACT

2. EXTRACT of a LETTER to Dr. CLARK from Mr. HORNE, Surgeon, dated Newcastle, February 28, 1791.

Query III.

About the twenty-fourth or twenty-fifth of October 1790, I inoculated four children in one family, none of whom, I was assured, had ever been affected with the small-pox. The youngest child's age was about two years and a half, the eldest nine years, the others of course between these ages. I took the matter from a child in the neighbourhood, who had the disease in the natural way and was very full. The spots on the face had run together, those on the body and limbs were tolerably distinct though very numerous; and, as well as I can remember, it was about the thirteenth day from the eruption. The pustules on the face were scabbed, and I took the matter from

from the legs, thighs, and feet, where I opened a great number of pocks, which were filled with a thick matter and in such plenty as to soak thoroughly a thick piece of scraped lint. I went next day and inoculated the *four children*. My common method (and which I then used) is to scrape up a quantity of matter from the lint, on the blunt part of a lancet, which I give to an assistant. I then dip the point of another lancet into the matter thus collected or scraped up, so as to have a small drop on the end of it. I make a puncture between the two skins without (if I can help it) drawing blood. I keep the point of the instrument in this situation, some little time, and, before it is withdrawn, I press the edge of my nail on the skin, so as to leave the matter within the puncture, when I take out the lancet. I use this method always on both arms. In all these children no appearance of the disease ever took place; though the house they

D d

lived

lived in was surrounded with others where it prevailed, and they were constantly playing about the doors. The punctures inflamed like a scratch; they all festered, as it is called, with a red angry appearance, and were covered with large scabs, which did not come off for some time. The father of these children is named Joseph Bengall, and he lives at the Mushroom glass-houses.

‘ At the same time, and with the same matter, I inoculated the child of the Rev. Mr. Turner, about four months-old, and here it also failed. I inoculated him again after the first punctures were healed, with fresh fluid matter, taken from the inoculated puncture of a child, then sickening and brought into the same apartment, but without effect. I should have remarked, that I inoculated three children near Joseph Bengall's, with matter taken from the same child, whence I took that which I used for his children,

dren, and they all had the disease favourably. But it may be right to mention, that it was taken about the eighth or ninth day from the eruption.

Query IV.

‘ With respect to the fourth query I must let the following circumstance give the answer. I inoculated the child of Mr. Snowdon Hatten on the twelfth of November 1790. The punctures inflamed in the usual manner yet rather tardily. About the eighth or tenth day he was restless and feverish, though but very slightly, and had not in all above six or seven spots just like pins’ heads. The punctures formed two fine pustules, filled with clear lymph and scabbed off in the common way. On the eighth of December a child of Mr. Barber, attorney, was attacked with the natural small-pox, of the very worst kind, and died in a few days. Mrs. Barber was well assured that the

child had never been exposed in any degree to the disease, except some days before, I think she mentioned about a week or ten days, when she had been drinking tea in a room where Mr. Snowdon's child was brought. But she was sure her child was never near the other, for her fears made her very cautious, and at that time Mr. Snowdon's had not the least mark of the disease. Indeed it had been from the first so very little affected, that its parents can hardly believe it has had the small-pox at all.

Query V.

' I can answer your fifth query, by saying that in my own practice, and at a rough guess, I have inoculated at least four hundred in the last eight years, I never had the least reason to think I communicated any infection from my person or clothes, though I have carried in my pocket a phial containing the matter on lint, and have not changed my dress

dress before I have visited uninfected houses. I have indeed sometimes used that precaution, yet it was but very seldom. Mr. Humble, I believe, thinks that he brought the small-pox to his own child, either by his clothes or person; but I must refer you to himself for the particulars.*

A case lately occurred to me which has puzzled me very much. A very beautiful girl, twenty-five years of age, servant to Captain Morton, had never had the small-pox, and had the most dreadful apprehensions of that disease. On the twentieth of January 1791, about four in the afternoon, she was standing near the kitchen fire, when a joiner in the neighbourhood came to the door, which is about sixteen feet from the fire-place where the girl stood. Mrs. Morton found fault

* See p. 385, where the same case is mentioned, and the suspicion refuted.

with the man for not coming sooner to finish some work, and he excused himself by saying his apprentice was ill of the small-pox, which had delayed him. The girl immediately clasped her hands and exclaimed, God forgive you, but I will lay my death to you. From that moment she became chilly, then hot and restless. She passed a very bad night, frequently exclaiming, God forgive Calder, he has killed me; meaning he had given her the small-pox. In the morning of the twenty-first, I was sent for, and found her very hot, with a quick pulse, great sickness and anxiety. I ordered her an emetic, and assured her she did not need to be in the least alarmed, as she could not possibly have caught the disease. She seemed to be convinced that her fears were groundless, but next day, the twenty-second, a violent rash appeared; on the twenty-third the small-pox came out, of the worst kind I had ever seen, and she died on the ninth day from the eruption.

‘ On

‘ On the most diligent inquiry I cannot find that she had ever been exposed to the disease, no other person being ill in the neighbourhood. The house where the apprentice lived was at a considerable distance from Captain Morton’s, nor had the young woman ever been near it. The joiner she assured me had never approached nearer to her, than the door, and he must have come from near the Ouse Burn Bridge, after leaving his own house, before he came to her master’s. I have since heard, that she had walked through Dentshole, where it was reported the disease prevailed, but she was certain she had never seen any person with it, or felt the least bad smell.

‘ I have stated this case exactly as it happened, and must leave it to you, to draw your own conclusions from it. For my own part, I cannot conceive it possible that the powers of the imagination could produce so specific a disease.

as small-pox; and one can hardly think the joiner could have brought enough of the effluvia on his clothes to have infected the girl whom he never approached nearer than I have mentioned, and had come about a quarter of a mile through the open air.'

3. EXTRACT *from the ANSWER to Dr.*
CLARK, *dated Chester, March 25,*
1791.

Accept my hearty thanks for the highly interesting and instructive publick letter, with which you have favoured me. I shall confine this answer to a few points of doubt or explanation; but shall reserve the important inferences, which appear clearly deducible, from the facts you have stated, to be introduced in the general recapitulation, or *Conclusion of the SKETCH.*

Query

Query I. It is a point of consequence to ascertain, at what period of the distemper a variolous patient begins to be infectious. On this account, I am well pleased with the minute detail of cases, you have given, relative to that point. Our inferences, from the facts which have occurred to us, are, perhaps little, if at all, different from each other. It is concluded (p. 49—52 of the INQUIRY) from instances which had occurred to Dr. HEBERDEN and myself, that “one
“who never had the small-pox might
“safely associate, and even lie in the
“same bed with a variolous patient,
“for the *two* or *three* first days of the
“eruption, without receiving the infection.” You say, “I am persuaded that many lives may be saved, by
“inoculating children, who have been
“daily exposed to the natural small-pox, during the *four* or *five* first
“days of the eruption.” My conclusion is exactly the same, though the facts

facts I have stated, lead me to it in a different manner. For (in the INQUIRY, p. 37.) it is observed; "hence
"it would seem, as far as these few facts
"bear evidence, that the period between
"infection, and the commencement of
"the variolous fever is about *two* days
"longer in the casual than the inocu-
"lated small-pox."

From various considerations, I am persuaded that the two different modes of infection take effect at different periods, according to the idea first published by Baron DIMSDALE, an author to whom the modern improvements in inoculation have many and great obligations. In the year 1779, I enjoyed the favour of a very free private disquisition with the Baron, on this discovery, two years before it was published. His facts and arguments convinced me that the period which succeeded inoculation was the shortest. And I endeavoured, at some length, to persuade him that
the

the period which succeeds casual infection was less than "from thirteen to twenty days, or more;"—but without success.

In the *Rules of prevention*, the expression, "after the pocks have appeared," is rather equivocal. The period of eruption continues during three days. If I had said "*completely* appeared," which our facts lead us to say, we should very nearly agree in our conclusions. The reason why this word was omitted arose from the difficulty of determining when the eruption was *complete*. It is easier to ascertain when some, than when all the pustules have appeared. The point between us lies in the *expediency* of a popular Rule. As to our medical principles, there is hardly a shadow of difference between us. Under judicious inspection, the limits of danger may be approached with safety. These are instructive disquisitions, but so tedious and dry as possibly

possibly to beguile the sleepy reader. In my system, whatever concerns the *Rules of prevention* is highly important.

Query II. You think that an additional rule should be proposed, as necessary to be observed, by every family liable to the small-pox, in large towns. You recommend that “they wash or
“otherwise purify all kinds of goods,
“received from retail shops.”

You justly observe that shopkeepers conceal, as much as possible, every instance of the casual small-pox, which occurs in their families; and would be otherwise liable to transgress the *Rules of prevention*. In the present state of Newcastle, Chester, and other large towns, where the distemper is always present, and the contagion is neither feared nor avoided, by a great proportion of the inhabitants, no infamy nor even blame arises from the most careless and injurious dispersion of the poison.

But,

But, if the progress of infection were carefully watched from house to house, and if a conviction generally prevailed, that it need not be communicated, but through ignorance, or malice, the publick opinion will be totally changed. In a few years, even in large towns, the young generation might be preserved from infection till they have a knowledge and consciousness of the terrible danger and misery which threaten them from this calamitous pestilence. When it appears that they have the power, there will soon follow an anxious desire, to avoid the distemper. In these circumstances, even shopkeepers will observe instructions with the most perfect obedience. And it will not be difficult to arrange matters so as to accommodate both private convenience, and publick safety.

The first point will be to obtain immediate intelligence from the shopkeeper, when the distemper has appeared

peared in his family. It may be done *confidentially* to the INSPECTOR. There will be no fear or hesitation, in confiding an important secret to a medical man, who is daily intrusted with so many, which he never betrays, in the ordinary course of his profession. He may privately convey instructions; or, if needful, he may permit the shop-keeper to have a *temporary* INSPECTOR, of his own choice, as the plan allows.

Let us suppose the most dangerous case. Let the same person who nurses the small-pox patient, serve the customers. Even in that most difficult situation, all mischief may be avoided, if the nurse be truly solicitous to observe directions.

Let a loose garment be provided to cover every part of her dress, which the patient can touch. On going into the shop, this garment must be instantly slipped off; or changed for a
clean

clean one. Her hands must be wiped with a wet cloth, kept ready for the purpose. All possible injury may be prevented by this extemporaneous preparation and by other regulations that are easily practicable. It might be tedious, and to you superfluous, to detail minutely all circumstances, on the present occasion; but they might be explained, with great advantage, and success, in particular cases, by the COMMISSIONERS, DIRECTOR and INSPECTOR of the establishment, whose knowledge would be continually improving by extensive experience.

On this point, I will only make another remark. The purification should always take place *in the infectious houses*; so that no contaminated article should be allowed to issue from them. If this were not done, but the suspicious goods were to be washed and aired on their reception into clean houses, innumerable difficulties, and alarms must be the inevitable consequence.

The

The intelligence communicated to you by Mr. Horne is to my purpose. It shall be introduced under the same head as your own. The failure of inoculation in Joseph Bengall's children being extraordinary, farther inquiry may be made whether they have had the small-pox before or since October 1790. — If Mr. Barber's child was infected by Mr. J. Hatten's, which seems improbable, in the circumstances here related; yet he might retain variolous matter on his clothes or skin; whence infectious miasms would arise.

Query V. Your answer under this head is very important to my argument. But on the present occasion, I would only hint that the clothes, and persons of the gentlemen, who are supposed to have communicated the dysentery, are not entirely free from suspicion of retaining the poison, in a solid or liquid form. One of the gentlemen found the patient's "skin clammy, bowels inflated,

inflated, &c." and the other, Mr. Rayne, "was sent for often to administer opiates after loose stools, and "to dress a blister." No account is given, that even the hands were always washed, after *touching* the patients.

Consider how few miasms could adhere to clothes, from a short delay in the patient's chamber, and how many even of those must be dispersed by passing through two hundred and fifty yards (the distance in both cases) of fresh air. Dip a *wet* cloth in a solution of emetic tartar, and drag it through two hundred and fifty yards of fresh water. Do you think, that, after this process, it could retain any emetic quality? Between such near neighbours, contaminated clothes might be conveyed in various unsuspected ways.

Enclosed 4. An

4. *An EXTRACT from the second LETTER of JOHN CLARK, M. D. Fellow of the Royal College of Physicians at Edinburgh, dated Newcastle, June 13, 1791.*

' You are still persuaded, that the two modes of infection, namely, by inoculation, and by accidental contagion, take effect at different periods; and that the former appears about two days earlier than the latter.

' The facts mentioned in your INQUIRY do not appear to me to be sufficiently numerous to ascertain this to be always the case. And I can place little dependence on the opinion of inoculators, who draw this inference; because they have allowed patients to remain with others under a mild disease, for some time before inoculation, who yet afterwards

wards received a favourable small-pox. The whole, indeed, we are obliged to take upon their word, without being supported by circumstantial facts; by such, at least, as can warrant any reasonable conclusion.

‘ But I am farther induced to dissent from this opinion by the two following facts, which fell under my own observation, where no ambiguity could take place. In both these cases of the natural small-pox, the eruptive fever commenced so early as the ninth or tenth day, after exposure to the contagion. Compare them with the cases inoculated at the Newcastle Dispensary, communicated in my last letter, and arranged in a table: you will observe, that, in many of them, the fever began on the same and later days.

‘ In August 1773, the driver of the Edinburgh Coach stopped at Lauder, about seventeen miles from Kelso: and

from a near relation of his own, recovering from the natural small-pox, he carried home to his own child a cake of gingerbread. Nine or ten days after the child had eaten the gingerbread, it was seized with an acute fever; and in two days more the small-pox made its appearance. The child was much loaded, and with difficulty recovered. In the same narrow lane, and in the adjoining houses, several children, liable to the disease, lived; but none were infected except such as had personal intercourse with the family; and, for a long time, I found it no difficult matter to trace the disease.

The other case I should have sent you before, could I have ascertained the distance of infection, or had it not entirely depended upon my memory. It however is as follows. The fourth mate of the Talbot Indiaman, of which I was surgeon, upon the ship's arrival off Dover, went upon deck, and immediately

immediately was struck with horror at seeing a person (a smuggler) who came up the ship's side, with marks of the small-pox, and still had several scabs upon his face. He came down, pale and exceedingly frightened, to my cabin; I endeavoured to persuade him, as he had not touched the patient, and was several feet from him, that he was safe, and would not take the disease. I gave him a glass of brandy and water, and he seemed to be satisfied with my arguments. However in nine or ten days he fevered, and had a very large eruption of the small-pox, which were contiguous, but not confluent, and he recovered. I saw the person who gave him the small-pox, a nasty dirty fellow, who was much marked, and still had scabs upon his face. The person was to the windward of the young man whom he infected; but at what distance I cannot now say; although I think the young man had just got up the hatch way, and the fellow with the

small-pox was standing in the gang way. As the ship was sailing, the position of the sail might also be favourable for directing the effluvia. But for want of accuracy, the points making for all your purposes, I have it not in my power to ascertain.*

‘ Your observations on the additional rule I proposed, concerning the purification of goods from retail shops, convince me that it is unnecessary.

* Cases of this kind, free from all ambiguity, are so difficult to obtain, and so fairly lead to important conclusions, that I am glad to present another to the reader, and on the best authority. It was communicated to me, by Dr. BINNS of Liverpool. A boy, named Peter Patterson, arrived in England from the West-Indies, on August 29, 1786. He was sent to school on the thirtieth. In a room adjoining to the school was a child ill of the small-pox; whom he saw, but did not nearly approach on that day, and not afterwards. Peter's fever began on the eleventh of September, early in the morning; not quite twelve days from the time of being exposed to the contagion. The pilot was taken on board in the afternoon of the day, before the vessel arrived.

‘ In

‘ In answer to your fifth query, I gave you two instances, where infection was carried home by medical attendants. But it is impossible to say that *matter* might not, in both instances, be taken home in a liquid form, on the clothes. The first gentleman, most assuredly examined the inflated bowels of the patient, under the bed clothes; and the other dressed a blister. Consequently the garments of both might contain more than mere effluvia.

‘ Mr. Horne informs me, that he has inquired into the state of Joseph Bengall’s family. The parents assure him, that none of the four children had the least marks of the small-pox previous to inoculation, nor have been since affected with the disease.’

IX. EXTRACT of a LETTER from Dr.
DE LA ROCHE of PARIS, *communicating* Answers to the QUERIES, from
Dr. ODIER of GENEVA.

' The practice of physick in a large town supplies no convenient resource for making such observations as your researches on the contagion of the small-pox would require, and Paris is perhaps a worse place for this purpose, than any other. Prohibitions to inoculate within the town, were made many years ago, and their effect is still kept up by the prejudice of all ranks of people, in regard to the danger that must (according to them) necessarily attend that practice. However, as I am desirous to contribute all in my power to help you in throwing light on so great and useful a subject, I have
sent

sent your letter to my friend Dr. Odier, of Geneva, who has done more than any body else, in that place, to promote inoculation, and whose observations deserve all trust, as being always most accurate and judicious. I will transcribe what he has written in answer to your letter.'

“ S'il s'agissoit de répondre théoriquement aux questions du Dr. Haygarth je pourrois probablement le faire comme un autre. Mais par des faits cela m'est impossible, à moins que de me renfermer dans des généralités qui ne donneroient qu'une foible probabilité. Vous savez aussi bien que moi qu'il n'y a rien de plus difficile que de faire des observations précises sur la manière dont se propagent les maladies contagieuses. Celles, qui comme la petite vérole, sont non seulement contagieuses mais encore par fois épidémiques, présentent encore plus de difficulté, parcequ'il arrive fréquemment

ment que les circonstances capables de propager la contagion pendant l'épidémie deviennent insuffisantes pour cela dans les années d'intervalle. Voici à peu près à quoi se réduiroit tout ce que je pourrois répondre aux cinq questions de la p. 120 des RECHERCHES, en me bornant à ce que j'ai vu.

“ 1. Question. Les Rèlemens donnés p. 105. ne prescrivent ils aucune précaution inutile ?

“ Réponse. Nous avons fréquemment inoculé à Genève un grand nombre d'enfans dans des années pendant les quelles la petite vérole n'étoit pas épidémique. Ces enfans sont sortis tous les jours, même après l'éruption faite. Ils ont été dans les rues & dans les promenades publiques; ils ont communiqué librement avec d'autres enfans susceptibles d'infection, & non seulement la petite vérole ne s'est pas répandue, mais encore il n'est parvenu

à ma

à ma connoissance aucun exemple bien prouvé de communication de la maladie d'un individu à l'autre dans la rue, ou à la promenade, d'où je conclus que le second article des réglemens paroît jusqu'à présent inutile. Je voudrois au moins en ne consultant que mon expérience le restreindre beaucoup pour ne pas trop gêner l'inoculation.

“ 2. Q. Prescrivent ils toutes les précautions nécessaires ?

“ R. J'en doute. Car j'ai souvent vu des malades de petite Vèrole qui avoient pris la maladie sans qu'on pût decouvrir aucune trace directe ou indirecte de contagion. Mais quoi qu'il paroisse probable que la petite vèrole peut se communiquer par des voyes jusqu'à présent inconnues, il ne me semble pas qu'on puisse indiquer aucune précaution à ajouter à celles qui sont mentionnées dans le Règlement. Il y a quelques années que
je

je vis une Dame de 45 ans qui toute sa vie avoit été dans la plus grande appréhension de prendre la petite vérole, & qui avoit fui avec le plus grand soin toute espèce de communication directe ou indirecte avec des malades. Elle la prit cependant au moment ou elle s'y attendoit le moins. Je me donnai beaucoup de peine pour tracer la contagion à sa source; je ne pus rien découvrir. Il n'y avoit alors aucune épidémie dans la ville, & rien de ce qui l'avoit touchée ne paroïssoit avoir été en contact avec aucun malade. Elle l'eut fort heureuse, mais sans jamais savoir de qui & quand elle l'avoit prise.

“ 3. Q. Avéz vous jamais vu trois personnes &c. &c ?

“ R. J'ai vu deux enfans du même père & de la même mere, agés de douze à quatorze ans qu'on m'affuroit n'avoir jamais eu la petite vérole, & être

être du côté de leur mère d'une famille qui n'en paroïssoit pas susceptible, puisqu'aucun des individus de cette famille ne l'avoit eue. Je les inoculai trois fois, dans le même lieu, dans le même tems, du même venin, de la même manière, avec du pus frais, & toujours inutilement. Ils ont communiqué depuis avec des malades sans en être infectés.

“ 4. Q. Avez vous jamais vu la contagion de la petite vérole &c. ?

“ R. Je ne puis repondre à cette question. En pareil cas, on ne peut être sur de rien. Mais en dernier lieu un enfant a pris la P. V. à Genève sans qu'on pût tracer d'autre source de contagion que le voisinage d'un enfant inoculé dans la même maison & au même étage, mais dans un appartement différent. On disoit n'avoir eu aucune communication.

“ 5. Q.

“5. Q. N'avez vous pas vu souvent au contraire, &c ?

“R. Cela se voit tous les jours & nous en avons dans la pratique des exemples innombrables, quoique, d'un autre côté, les exemples de communication par un pareil moyen ne sont pas rares.

“Voilà tout ce que je puis dire de positif sur ces questions. J'ajouterai sur la troisième que j'ai souvent observé dans l'inoculation une grande différence de susceptibilité d'infection entre divers individus, sans pouvoir en assigner aucune cause. En inoculant plusieurs enfans à la fois, dans le même lieu, du même pus, avec la même lancette & de la même manière à tous égards, il m'est souvent arrivé de voir quelques uns de ces enfans, ne pas prendre la petite vérole à la première inoculation, tandis que les autres la prenoient fort bien. Mais ceux qui ne la prenoient pas à la première fois la prenoient

prénoient pour l'ordinaire à la seconde. En inoculant avec du pus frais le succès est plus assuré. Du pus sec & délayé est aussi moins efficace à proportion de son ancienneté, à tel point qu'au bout de deux mois il ne reussit plus. Cependant il m'a été impossible jusqu'à présent de vérifier les limites au delà desquelles, il est absolument inerte. Seulement je suis assuré qu'il y a entre le pus liquide & récent & le pus sec, différentes nuances d'efficacité, tellement que le pus desséché ne perd que graduellement son activité, & peut encore avant de l'avoir entièrement perdu, infecter un ou deux sujets sur dix, ou peut-être sur cent, tandis que s'il étoit frais & liquide, il pourroit en infecter neuf sur dix & peut être plus. Mais sur plusieurs sujets inoculés à la fois qu'est ce qui détermine l'infection des uns par preference aux autres c'est ce qu'il m'a été impossible de découvrir."

"I wish this abstract of my friend's letter may be agreeable to you. I
regret

regret much that I am not in such a position as to make observations you might think worthy to be added to those you have already collected; but in any thing I may be of service to you, depend on my readiness.

TRANSLATION

OF DR. ODIER'S ANSWERS TO THE QUERIES.

“ If I had been desired to answer Dr. Haygarth's queries theoretically, I might probably do it as well as another. But it is impossible from facts, unless they be given in so general a manner as to convey but a feeble degree of probability. You know, as well as I, that there is nothing more difficult than to make accurate observations, on the manner by which contagious diseases are propagated. Those which like the small-pox are not only contagious, but, at times, epidemical,
present

present yet greater difficulty : because it often happens, that the circumstances capable of communicating contagion during the epidemick, become insufficient for the purpose in the intervals of these periods. This will be nearly all I can say in answer to the five queries proposed in the one hundred and thirty-fifth page of the INQUIRY, in confining myself to the facts which have fallen under my own observation.

‘ I. Query. Do the *Rules of prevention* contain any unnecessary restriction?

‘ Answer. At Geneva, we have frequently inoculated a great number of children during the years when the small-pox was not epidemical. These children went abroad every day, even after the eruption had appeared. They were sent into the streets, and the publick walks, and have communicated freely with other children, who were

F f susceptible

susceptible of infection. Yet the small-pox did not spread by this intercourse: there even did not come to my knowledge an instance clearly proved, of the infection being communicated to a single individual, either in the streets, or the publick walks. Whence I conclude that the second article of the Rules appears to be superfluous. I would, at least, in only consulting my own experience, make it much less strict, lest an inconvenient restriction may be imposed on inoculation.

“ 2. Query. Do they contain every necessary restriction.

“ A. I doubt it. For I have often seen patients attacked by the small-pox, without being able to trace any direct or indirect means, by which the contagion was communicated. But although it appears probable that the distemper may be communicated by means hitherto unknown, it does not follow that

that one can point out any precaution to be added to those which are mentioned in the Rules. Some years ago, I knew a lady forty-five years of age, who had all her life entertained the greatest apprehensions of being attacked by the small-pox, and who avoided, with the greatest care, every kind of communication, direct or indirect with such patients. She was, nevertheless, seized with the distemper at a time she least expected. I took much pains to trace the contagion to its source, but could not discover it. The distemper, at that time, was not epidemical in the town, and nothing which she had touched, appeared to have been in contact with any patient. She had the disease very mildly, but without ever knowing from whom, and when she had taken it.

"Q. 3. Have you ever seen three persons, &c.

F f 2

A. I have

' A. I have seen two children of the same father and the same mother from twelve to fourteen years of age, who, I was assured, had never had the small-pox, and who were on the mother's side, of a family incapable of receiving it, because not a single individual of that family had ever had it. I inoculated them thrice in the same place, at the same time, with the same varicellous matter, in the same manner, with fresh pus, and always unsuccessfully. They have since had intercourse with patients in the small-pox without being infected.

' A. 4. I cannot answer the fourth query. In such a case, one can be certain of nothing. But a child took the small-pox at Geneva without our being able to trace any other source of contagion than the vicinity of an inoculated child in the same house, and on the same story, but in a different apartment.

apartment. They were said to have had no intercourse with each other.

‘ A. 5. One sees instances of this kind every day, and in the course of practice we have innumerable examples: but, on the other hand, cases where infection was communicated by these means are not uncommon.

‘ This is all I can say positively in answer to the queries: concerning the third, I will add, that I have often observed in inoculation, among various individuals, a great difference of their susceptibility of infection, without being able to assign the cause of it. In inoculating many children together, in the same place, with the same pus, with the same lancet, and in all respects in the same manner, it has often happened, that some of these children did not take the small-pox from the first inoculation, while others were infected.

F f 3

But

But those who failed from the first operation, generally received it from the second. When fresh matter is employed, success is more certain. Dry and diluted matter is also less efficacious in proportion as it is older, so that, at the conclusion of two months, it becomes totally inefficacious. Although it is impossible accurately to ascertain the limits when it is absolutely inert, I am convinced that there are different degrees of efficacy between the liquid and fresh, and the dry *pus*; so that the driest matter only loses its activity gradually, and can, before it be entirely lost, infect one or two in ten, or perhaps in a hundred. Whereas liquid fresh matter can infect nine out of ten, or in a greater proportion. But when many patients are inoculated together, it is impossible for me to discover why some are infected rather than others.'

X. EXTRACT

X. EXTRACT of a LETTER from
JAMES CURRIE, M. D. Fellow of
the Royal College of Physicians at
EDINBURGH, dated Liverpool, Nov.
28, 1791.

‘ I thank you for the perusal of the printed sheets of your new work on the small-pox, which I really think very valuable, and which I doubt not will procure you an increase of reputation among the enlightened and humane.

‘ In the ingenuous spirit which I see you encourage in your Correspondents, I shall proceed to make a few remarks on the subject of your *queries*, and afterwards offer you such general observations as I think of any importance.

‘ To your first and second questions respecting the efficacy and the simpli-

F f 4

city

city of your *Rules of prevention*, I answer, that I am persuaded they contain more restrictions than the object in view commonly requires, though I do not know that any of them could be safely omitted; that they contain all I incline to think necessary *in any case*, but that I do not conceive we are absolutely *certain* of this. I believe, however, that they are as perfect as our knowledge admits, and such as ought to be acted on.

‘ To your third question, I answer, I have never seen an instance of three persons exposed together to the contagion, and capable of receiving it, who have all escaped the disease: and to your fourth and fifth, that I cannot from my own observation, mention with certainty, an instance of any persons carrying the contagion from one room to another merely by imbibing variolous miasms from the air into his clothes; while, on the contrary, the instances

instances of persons being exposed to the air of a room tainted with variolous effluvia and not propagating the disease, are very numerous.

‘ The particulars of the following case, into which I have taken some pains to inquire, are uncommonly well authenticated. A young lady now of this town, was inoculated in her fifth year. Her arm inflamed and suppurated, and though no pustules appeared on her body, yet having been supposed to have had some fever, it was universally concluded that she had passed through the disease. In this belief, she went soon afterwards with a near relation to visit a poor child in the neighbourhood, in the height of the confluent small-pox. The weather was warm, the room small, the variolous effluvia extremely offensive: she continued in this apartment half an hour, at a small distance from the patient, probably about five or six feet, but

but escaped unhurt. Three months afterwards she went with the same lady (her aunt) and a stranger to pay a morning visit in this neighbourhood; the family were from home, but a child, two years of age, in the inoculated small-pox, was in the house. The disease had been very mild, not more than thirty pustules having appeared; it was two days past the turn, and the infant boy followed the party, who did not sit down, but walked through several very lofty and spacious apartments. It is pretty certain that the young lady touched the child, and it is very probable that she kissed him. Ten days afterwards, she was seized with the eruptive fever and had the disease in a very dangerous form. This case seems to prove that, even in those instances of small-pox, where the disease is most severe, and the offensive smell spreads widely, the really contagious atmosphere extends to a very small distance. The inspection in the
second

second instance of exposure, from a very mild case, shews the effects of nearer approach, or actual contact.

I could also bring many facts to prove that the contagion has spread a very little way into the atmosphere in situations where many patients have been confined together and consequently the quantity of effluvia greatly multiplied. These are chiefly from the accounts of our Guinea voyages, in which the small-pox used formerly to make, at times, dreadful havoc among the slaves. The practice, however, of late years has been, immediately on the appearance of the casual disease, on ship board, to inoculate the whole cargo, and nothing can speak more forcibly the safety of inoculation, than the complete success with which it has generally been attended on persons of all ages, entirely unprepared, and under circumstances every way unfavourable. As however a general inoculation under such circumstances

stances is always followed by unpleasant, and, sometimes, though rarely, by destructive consequences; it is not now uncommon to separate the diseased persons, and to trust to means of prevention for the safety of the rest. These frequently, perhaps I might say generally, succeed, provided the voyage is performed with light and favourable winds; which is necessary to enable them to make the separation complete. An instance of this has just occurred, in a Guinea-man, called the Golden Age; soon after she left the coast of Africa, the small-pox appeared, and, before the disease was known, eight persons were affected; the whole were immediately brought on deck, their apartments washed and ventilated with the greatest care, and the eight persons ill were placed in the main-top about twenty feet from the deck, where they regularly passed through the disease. Before coming down, they were washed, the contagion was extinguished, and

and the whole cargo, as well as crew, arrived in perfect health in the West-Indies. During all this time, the slaves, as is usual, passed the day on the deck below, but though all were supposed liable to the disease, not one of two hundred and upwards thus exposed, received the infection. My friend Mr. Beg, formerly surgeon, afterwards master of a Guinea-man, and now a considerable merchant here, informs me, that, in one of his voyages, he practised the same method of stopping the contagion of this disease, and with the same success. He acquaints me also that twice, when the small-pox appeared among the slaves, while they were at anchor on the coast, he put the infected persons in a boat a-stern of the ship, and effectually secured the people on board from the contagion. Many similar facts might be collected, and, as I see they may illustrate and enforce your doctrines, I will attend to the subject.

On

‘ On the question, concerning the length of time which variolous matter exposed to the air retains its infectious quality, I beg leave to mention the following facts, I cannot call them experiments.

‘ About two years ago, being requested to perform inoculation (a thing not uncommon to physicians here) I applied to my friend Mr. Park for some fresh variolous matter. He told me he had none, but that he would lend me his glass on which he had spread some, taken from a patient about a month before, and with which he had since several times inoculated. Looking at the glass, a small magnifier, I could observe it stained with matter, but perfectly dry, and Mr. Park informed me that it would be necessary to drop a little warm water on the glass, and mix it up with the dried matter. I did so, and produced a milky liquid, in which I dipped the point of
a lancet,

a lancet, and communicated the disease in the usual way. I collected a small quantity of variolous matter, some time after this, on a glass of my own. Being exposed to the air it soon became dry, and I did not use it for two or three weeks. By adding a little water, the variolous matter was however liquified and inoculation performed with success. I laid the glass aside, and it was again soon dry; but, about a month afterwards, I inoculated with it in the same way as before. It now lay by seventy-three days. When I looked at it, the stain on the glass was broad, but very faint; I was doubtful that it would not again succeed in the same way; but, an old servant applying to have a child inoculated, and having no other matter, I added a little warm water, as formerly, and mixed up a liquid resembling whey. With the point of a lancet dipped in this, I succeeded as perfectly as before.

‘ The

‘ I took the matter on a microscope made by Dolland. The glass shuts between two covers, by a hinge, in the usual way, but was accessible to the air, all around, on both surfaces. It generally lay on the chimney piece of my study, under which there was a fire, about six weeks of the time. From these curious facts I do not infer, that the air takes up no variolous matter at all. My notion is, that a portion of it rises by evaporation with the water in which it is diffused. But that, after the water is evaporated, a residuum remains, not soluble, or *soon* diffusible in air, and therefore, while dry, incapable of communicating infection to a by-stander. This residuum, however, when diluted with water, becomes capable of communicating infection, by inoculation, and probably also, by tainting the air. I shall soon have an opportunity of trying, in part, the validity of this opinion, by exposing variolous matter in different circumstances,

stances, to the external air, so that the evaporation may be promoted, as much as possible. This question shall thus be brought to the test of actual experiment. If you will propose any rules to be observed, they shall be strictly attended to.

‘ To the plan which you have laid down in the preceding pages for the extinction of the small-pox, I see no other objection than the difficulty of the execution, for I doubt not that it is theoretically perfect. Unfortunately, it requires the assistance of government, and government is not in the habit of attending to such objects. Our governors indeed all inoculate their children; so do almost all those who have influence upon them: they of course themselves escape the danger of the disease, and it is curious to see with what surprising firmness men bear the misfortunes of their neighbours! You have however done your duty, in

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showing how a dreadful scourge may be removed from among us, and you may cheerfully trust the rest to that moral Providence whose operations on the mind of man are every day more strikingly displayed.

‘ One effect of your plan is, that you will set other people a thinking on the subject, and that other plans for the same purpose will probably be proposed.—Will you permit me to mention one suggestion of my own?

‘ If a law were made confining the performance of inoculation to two periods in the year, namely, from the first to the tenth of October, and from the first to the tenth of April; and if a provision were also made for the inoculation of all the children of the poor who could be induced to consent to it at these two periods (a thing that might be done with very little expense, by means of the present charitable establishments

blishments and the assistance of the clergy) the consequence would be, that the rich, for their own safety, would inoculate at such periods also, and those who did not inoculate, being put on their guard, would use those means of precaution which you have shewn to be so easy and so effectual. The safety of this practice would soon overcome the fears of the weak, and shame the worthless into a conformity, which their sense of duty or feelings of humanity could not produce.

‘ Something of this kind, we found in our first general inoculation at Liverpool in October 1781. The affluent being alarmed at the advertisement for this purpose, presented their children for inoculation also in great numbers, and a hundred and sixty-one passed through the disease. Those who thought their children improper objects for inoculation, took measures to prevent their being infected.

So effectual were these, though conducted on no exact system, that when the general inoculation was completed, the disease was actually extinguished in the town; and, six weeks afterwards, when a child of some distinction was to be inoculated, no variolous matter could be found in Liverpool, and it was imported from a distance. This fact is highly important. It points out the advantage of the law I have mentioned, till something more complete is adopted, in a clear point of view: and illustrates your doctrine of the narrow sphere of contagion in a striking manner. There were four hundred and seventeen persons inoculated in all, in every part of the town, by the associated inoculators; and perhaps a hundred more, at the same time, by other practitioners. Three or four thousand liable to the disease were scattered in the same manner, not one of whom caught the infection. I ought to mention that we sent round printed hand-bills from house

house to house, copied from your regulations at Chester, to point out the means of guarding against it. I lament much that our plan for general inoculations is dormant at present, but I hope it will be revived. Our experience, for several years, was uniformly in favour of its utility.

‘ An important particular has been recalled to my mind by Mr. Park; that, previous to this first general inoculation which extinguished the small-pox, in so extraordinary a way, the disease raged in town with much violence, and was very fatal.

‘ Whether your proposals will meet the general attention they deserve I will not presume to say. I have my doubts whether knowledge and philanthropy are as popular at present as they ought to be. In the mean time, however, your book will receive the approbation of those who are judges

of its merit, and its principles will doubtless be called into action, if the happy period so confidently foretold should ever arrive, "when the powerful shall be subjected to the wise."

2. *An EXTRACT from the ANSWER to Dr. JAMES CURRIE, dated December 16, 1791.*

The principal instruction deducible from your very judicious letter will be introduced, with most propriety, in the conclusion of the SKETCH.

I am very glad to learn that you are resolved to determine, experimentally, how soon variolous matter is deprived of its infectious quality, by exposure to fresh air. Your observations may perhaps verify a conjecture which I had

had hazarded in the INQUIRY (p. III.) that the atmosphere might not be able, in certain circumstances, to deprive the variolous poison of all its infectious quality, but that a part of it might be shut up by dryness on its surface.

The true question is this; for how long a time does the recent poison contaminate the air, so as to produce the casual small-pox? Is this quality renewed by moistening the matter? or by heating it? As an actual trial, in so dangerous a distemper, cannot be warrantable, may we deduce a probable conclusion, from the cessation of the variolous smell? and from its renewal by moisture? and by heat?

As this method of investigation cannot prove satisfactory, the similitude between inoculated and casual infection, being so great, might we fairly infer that the matter which generates

the one would generate the other? A cavilling hypothesist, indeed, might answer; 'no, the volatile and fixed parts of the poison produce diseases of totally different kinds.'

But this and every other doubt may, I think, be completely removed, by subjecting the variolous poison to some experiments with an accurate balance. Mr. Ramsden, you know, has improved this instrument so as to turn with the $\frac{1}{600}$ part of a grain. I am possessed of a balance made by this eminent artist, and if you will undertake the following experiments, with others which will occur to you, it is at your service, for the purpose.

In order to ascertain, in a satisfactory manner, what proportion of the poison is soluble in air, by exposure to the atmosphere, to moisture, and to heat; let variolous serum, pus and scab be separately collected in as large a quantity

a quantity as possible; not less than several grains. On their removal from the patient, they must be instantly weighed, or instantly shut up from the air in a close vessel, and weighed in that state. They may then be exposed to a current of air, in a moderate temperature, which ought to be noted. The loss of weight, in each second, minute, hour, and day might be ascertained: it will probably be fastest in the serum, and slowest in the scab. After perfect dryness, do they continue to waste?

Again, let the dry serum, pus, and scab be well moistened with pure *distilled* water. After evaporation to dryness, without heat, are they lighter than before they were moistened? If they are, may we not conclude, that some of the poison has been communicated to the atmosphere along with the water? On the contrary, if the same weight remain, after evaporation, as before

before the addition of water, may we not fairly maintain that moisture does not contribute to the contamination of air, by the variolous poison? To clear up all doubts on this point, such an easy experiment might be repeated ten or twenty times.

It can hardly be doubted that the variolous poison, in every form, will totally evaporate by heat. And we may deduce some instructive inferences from experiments, to ascertain, at what degree of the thermometer this evaporation takes place. If this point were accurately determined, as it easily may, heat might, in some cases, be the cheapest and most expeditious method of purification. It might perhaps be satisfactorily proved, what seems not improbable, that a poor man's coat can be as perfectly disinfected by a hot fire or oven, as by washing it: and the former process would be more conveniently accomplished than the latter.

Take

Take two similar pieces of cotton. Weigh them accurately before and after they had been equally dried by a hot fire. Soak one of them with several grains of variolous serum or pus. Weigh it in that state; and determine the degree of heat and its continuance, by which the piece of cotton is restored to its originally pure state. Every particle of the poison has weight, and Ramsden's balance will detect a very minute fraction of it.

The conclusions that may be deduced from these experiments, can remove every *possibility* of doubt. They admit of the minutest accuracy; both in regard to the balance, and by the continuance of time which may be employed in the trial. If, in given circumstances, the variolous matter does not waste $\frac{1}{600}$ part of a grain, in a month, how little can it lose in a minute? And air is a fluctuating body which changes its situation every minute.

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I am particularly solicitous that you should undertake the experiments you have proposed, as well as those which I have here taken the liberty to suggest. They cannot fail, I think, to improve the *Rules of prevention*, an object in my estimation, of high importance.

3. EXTRACT of Dr. JAMES CURRIE'S
second LETTER, dated April 1, 1792.

‘ It has not been in my power to attend to the experiments you suggested, but I have tried whether variolous matter exposed to the air, and indeed to the wind for thirty days can communicate the disease by inoculation, and the result has been as I expected; the disease was communicated with the usual certainty and success. I inoculated three patients at the same time: in one, the eruptive fever appeared on the fifth day,

day, in another on the seventh, and in the third on the ninth. There were no peculiar circumstances, the disease being in every case perfectly mild. The matter was spread on the surface of a piece of window glass, and had been long perfectly dry. It was discoloured by some smoke and dust that the wind had blown over it. I diluted it, as our practice is, with a few drops of tepid water, and restoring it nearly to the liquid state in which it was originally taken, performed the inoculation by introducing the point of a lancet dipped in it under the cuticle, as recommended by Baron Dimf-dale, which is my usual way. Next day, the glass was as dry as before, and the matter unchanged as to appearance. It now lies before me, and the eye can distinguish no diminution of quantity. So far as to facts; my opinion is that I shall be able to inoculate with the same matter diluted in the same way many months or perhaps years hence.

The

The reason why variolous matter long kept has sometimes failed in producing the disease, I apprehend to have been, that the dried matter was not previously reduced to a state of fluidity. The point of a lancet, stained with variolous matter, long dried (or a thread tinged in the same way) introduced under the skin, is probably withdrawn without leaving any of the variolous particles behind; or, if some be left, these being in a solid state, are not acted on so easily, if at all, by the absorbents. This is agreeable to every analogy. The absorbents on the surface of the body at least, do not, in their natural state, act on solid bodies, which diluted or comminuted, they take up readily, as, mercury and sulphur.

‘ In order to judge what quantity of the solid variolous matter may evaporate with a given proportion of water, and also to judge to what degree the variolous matter may be diluted without losing

losing its power of communicating the contagion, several obvious experiments present themselves, which I will attend to, as well as to the other points you have suggested, as soon as my leisure will permit.

‘ I spread four grains of variolous matter on a piece of glass, which I exposed to the atmosphere. I weighed it some time after it was dry, and again at the distance of twenty-three days; and I was not able to discover the smallest diminution of weight. My balance is not indeed very nice, but capable of detecting the loss of half a grain and under.’

XI. I. *A LETTER from the SYNDICK
and COUNCIL of HEALTH of GE-
NEVA.*

Monsieur HAYGARTH, M. D. à CHESTER.

‘ Monsieur. La Noble Chambre de la santé établie dans cette ville, étant par les nouvelles loix de la Republique tenue de s’occuper de tous les objets qui peuvent intéresser la santé publique, et notamment des moyens d’empêcher l’introduction, et la communication des maladies épidémiques et contagieuses, a pris en considération les ravages que font les epidémies de la petite vérole, et les différens moyens qui ont été proposés pour les diminuer. Il étoit impossible, que dans la discussion de cet important objet, elle ne rendit pas à vos talens et au zèle avec lequel vous vous en êtes occupé dans votre patrie, l’hommage que méritent,

tent, et l'intéressant ouvrage que vous avez publié la dessus (ouvrage dont notre compatriote Monsieur le Docteur De la Roche nous a donné la traduction) et les succès de l'admirable établissement que vous avez formé à Chester. Aussi, Monsieur, je suis chargé de vous faire part de ses sentimens, et de vous prier de vouloir bien nous aider de vos conseils dans le dessein ou nous sommes, si la chose est praticable, d'en former un pareil à Genève.

Il faut que vous sachiez Monsieur, 1°. que la Republique de Genève a en tout 35000 habitans, dont 26000, ou environ, logent dans la ville, et a peu près 9000 habitent les Campagnes, et les villages voisins.

2°. Que la ville est d'une petite étendue, peu proportionnée à une population aussi considérable, sans qu'il soit facile de l'augmenter à cause des
H h fortifications

fortifications qui l'entourent. Elle n'a guères que 1200 maisons, qui, étant distribuées comme l'ancienne ville d'Edimbourg, en plusieurs étages, et en plusieurs appartemens, contiennent l'une dans l'autre à peu près 21 habitans par maison.

‘ 3°. Qu'une grande partie de cette population consiste en étrangers, non seulement parce que la plupart des domestiques, et des gens de basse professions nous viennent du dehors ; mais encore parce que Genève étant une ville frontière entourée, de toutes part, par la Savoye, la Suisse, et la France, et située, sur la plupart, des grandes routes de communication entre ces trois Etats, sert continuellement de passage aux voyageurs qui vont et viennent.

‘ 4°. Que malgré cet abord continu d'Etrangers dans nos murs, les Epidémies de petite verole font, chez nous,

nous, assez régulièrement périodique, de 5 an 5 ans, et que dans l'intervalle des Epidemies, il arrive fréquemment, que nous n'avons point de Petite vérole naturelle, ni dans la ville, ni dans les environs.

5°. Que l'inoculation a commencé à s'établir chez nous, des l'an 1751, que dès lors, on a constamment inoculé un très grand nombre d'enfans toutes les années, et avec un très grand succès, puis que la proportion du nombre des morts à celui des malades n'a guères été que d' 1 à 300 ; que quoique l'on ait souvent inoculé avec du pus importé de l'étranger, et dans un tems ou il n'y avoit point de Petite vérole, dans la ville, et quoique les enfans ainsi inoculés ayent librement fréquenté les rues, les promenades, et les places publiques, avant, pendant, et après l'éruption, on n'a point remarqué qu'ils repandissent la contagion, ni qu'ils produissent aucune Epidémie in-

tercalaire, ni enfin, qu'ils accélérassent le retour des Epidémies réguliers, et périodiques.

6°. Enfin, que les citoyens jouissent ici d'une constitution républicaine, dans laquelle le gouvernement est appelé à avoir les plus grands égards pour la liberté individuelle, aucun moyen coercitif tendant à empêcher l'introduction ou la communication de la petite vérole par la voye de l'autorité ne paroît praticable, et que nous croyons devoir nous borner entièrement dans ce but à des mesures adhortatoires ou à de simples précautions de police qui ne foyent ou ne paroissent point trop gênantes pour les individus.

Cela pesé, la Noble Chambre me charge, Monsieur, de vous demander.

1°. Si l'établissement que vous avez formé à Chester, continue, et avec quel succès.

2°. S'il

‘ 2°. S’il a été imité en d’autres villes de la Grande Bretagne.

‘ 3°. Si le gouvernement ou les administrateurs municipales y sont intervenus, et en quoi ou comment.

‘ 4°. Si dans les lieux où de pareils établissemens ont été formés, la position et les circonstances locales de ces lieux étoient très différentes des nôtres, et plus ou moins favorables à la réussite de ces établissemens.

‘ 5°. Quelle dépense annuelle a été nécessaire pour leur succès, tant à Chester qu’ailleurs.

‘ 6°. Enfin, que vous presumez, que nous pourrions faire à Genève, tant pour rendre l’inoculation plus générale parmi le peuple de la ville et des campagnes, que pour empêcher la contagion naturelle de se répandre ; il est douteux

que nous puissions former ici une Société particulière de contributables pour récompenser ceux qui consentiroient qu'on leur fit du bien à cet égard, il ne l'est pas moins que le Gouvernement dont les ressources pécuniaires sont très bornées puisse lui même faire de grands sacrifices dans ce but ; il est douteux même qu'il voulut consacrer le principe que les Peres et Mères qui évitent à leurs enfans les dangers d'une grande maladie ont besoin de récompenses ; mais ce qui ne l'est pas, ce qui ne sauroit l'être, c'est, que nous y mettrons toute la chaleur et tout le zèle que mérite l'importance d'un pareil objet.

• La Noble Chambre espère, Monsieur, que vous voudrez bien l'aider de vos lumières, & de vos conseils, en nous adressant votre réponse.

• Nous avons l'honneur d'être, avec une parfaite considération,

• Monsieur,

‘ Monsieur, vos tres humbles serveurs, le Syndic & Conseil de Santé de la Ville & Republique de Genève.

DES GOUTTES,

SECRETAIRE.’

LE 24, Xbre. 1791.

2. TRANSLATION *of a* LETTER
from the SYNDICK *and* COUNCIL *of*
HEALTH *of* GENEVA.

SIR,

‘ The Noble Chamber of Health, established in this city, being engaged, by the new laws of the Republick, to employ their attention upon all those objects which can affect the health of the people, and, especially, upon the means of preventing the introduction and communication of epidemical and contagious distempers; has taken into con-

H h 4 fideration

sideration the ravages made by the epidemical small-pox, and the different methods which have been proposed to diminish them. It was not possible, in the discussion of this important object, that the Chamber should fail to render due respect, to the talents and zeal with which you have been employed upon it, in your own country; to the interesting work, which you have published (of which work, our countryman *Dr. DE LA ROCHE* has given us a translation); and to the success of the admirable establishment, which you have formed at Chester. I am commissioned to communicate to you, Sir, these sentiments, and at the same time, to request that you would assist us with your advice, relative to the design we have formed to establish, if practicable, a similar institution at Geneva.

‘ It is proper to acquaint you, Sir, 1. that the republick of Geneva contains, about thirty-five thousand inhabitants;

bitants; of whom, about twenty-six thousand dwell in the city, and nine thousand in the neighbouring country, and villages.

‘ 2. That the city is of small extent, ill proportioned to so considerable a population; and yet it is not easy to enlarge the dimensions, on account of the fortifications with which it is surrounded. It consists of but few more than twelve hundred houses, which, being disposed like the ancient part of the city of Edinburgh, in many stories and in many apartments, contain, one with another, nearly twenty-one persons to a house.

‘ 3. That a great part of this population is composed of strangers; not only because most of our servants, and people who perform the inferior offices of society come from other countries; but farther, because Geneva, being a frontier city, surrounded on all sides by Savoy,

Savoy, Switzerland and France,* and situated on most of the great roads of communication between these three states, travellers who go and come, are continually passing this way.

‘ 4. That notwithstanding this constant resort of strangers within our walls, the epidemical small-pox is pretty regularly periodical, every five years; and that, in the interval of these Epidemics, it frequently happens, that we have no natural small-pox whatever, either in the city, or its vicinity.

‘ 5. That inoculation began to be practised here in 1751; that since that period, we have constantly inoculated a very great number of children every year, and with very great success; as the proportion of the deaths to the patients has but little exceeded 1 in 300. That although we have often inoculated with pus brought from a distance, and at a time when there was no small-pox

pox whatever in the city; and although the children so inoculated have freely resorted to the streets, walks and other publick places, before, during, and after the eruption; we never observed that they spread the contagion, nor that they produced any intermediate epidemick; nor that they even accelerated the return of the regular and periodical Epidemicks.

‘ 6. Lastly, that our citizens enjoy a republican constitution, in which government is required to pay the greatest regard to the liberty of every individual; that no coercive method to hinder the introduction or communication of the small-pox, by authority, seems practicable; and that we believe we ought entirely to limit our endeavours for this purpose, to measures of exhortation, or to simple precautions of police, which must not be, nor even seem to be, too troublesome to individuals.

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These circumstances being considered, the Noble Chamber directs me, Sir, to ask you,

‘ 1. Whether the establishment, which you formed at Chester, continues to exist, and with what success ?

‘ 2. Whether it has been imitated by other towns in Great Britain ?

‘ 3. Whether government or the city magistrates have interfered in the business, and in what manner ?

‘ 4. Whether, in places where similar establishments have been formed, the situation, and local circumstances were very different from ours, and more or less favourable to their success ?

‘ 5. What is the annual expence required for this purpose at Chester and other towns ?

‘ 6. How

‘ 6. How far do you presume that we can accomplish, at Geneva, our intentions, to render inoculation more general among the people of the town and country; and also to hinder the propagation of the natural contagion? It is doubtful whether we can form, in this place, a particular Society of subscribers to reward those who consent to accept the benefit we offer them. It is not less so, whether Government, whose pecuniary resources are very confined, can itself afford extraordinary supplies for this purpose. It is even doubtful whether they would sanction the principle, that Fathers and Mothers, who preserve their children from the dangers of a dreadful disease, require any recompense. But it is not, nor ought to be doubted, that we will pursue this object, with all the ardour and zeal, which its high importance deserves.

‘ The Noble Chamber hopes, that you, Sir, will have the goodness to
assist

assist them with your knowledge, and your counsel, by addressing to us your answer.

‘ We have the honour to be, with perfect consideration, Sir, your very humble servants,

‘ The Syndick and Council of Health for the City and Republick of Geneva.

DES GOUTTES,
SECRETARY.’

DECEMBER 24, 1791.

3. ANSWER

3. ANSWER *to the SYNDICK and COUNCIL of HEALTH, of the City and Republick of GENEVA, dated Chester, February 10, 1792.*

GENTLEMEN,

I feel, as I ought, a very high sense of the honour conferred upon me, by a letter from the Magistrates of a Republick, which, in proportion to the number of her citizens, is adorned with more men eminently distinguished for their knowledge in the sciences and in literature, than any other state of modern times. But I am still more delighted, with an account of your intention to form an establishment to prevent the introduction, and communication of the casual small-pox, and to promote inoculation within your territories. If such a measure be properly

perly executed, the benefits that may result from it to your and our country, and perhaps to other enlightened nations, must highly gratify every mind, which the sentiments of patriotism and philanthropy can warm.

I shall very explicitly answer the questions you have proposed; and, since you, Gentlemen, have condescended to ask my advice, on this important occasion, I shall give it with great freedom, not doubting that, what may be erroneously suggested, will be candidly corrected by your superior wisdom, and by your more accurate knowledge of various local circumstances.

When your letter arrived, I was engaged, as far as my professional duties would permit, in printing a SKETCH of a plan to exterminate the casual small-pox from Britain, in order to introduce general inoculation: and a

CORRESPOND-

CORRESPONDENCE on the nature of variolous contagion. As this work may contain some hints applicable to your proposed institution, I will send you a copy of the printed sheets immediately, and the remainder as soon as it is finished.

I. The Proceedings of the small-pox society at Chester, were suspended, soon after my former publication on that subject was sent to the press. This suspension was occasioned, neither by any medical difficulty, nor by a deficiency in the voluntary subscriptions, but solely by the ignorance and delusion of the populace.

Our plan was to propose gratuitous inoculation to the children of our poor fellow-citizens, every second year. At the close of the third period, when this favour was humanely offered to them, it was universally rejected. Though a clerical, together with a medical member of the society, very generally can-

vassed the whole town, yet not one, or but one child could be found, whose parents would accept the intended kindness. This vulgar folly was wholly unaccountable; for the general inoculations had been very successful, the proportional fatality not being greater than *one in two hundred*. In the district where I accompanied the clergyman of the parish, to make inquiry for proper patients, the only excuse which I remember to have been given for refusing the offered boon, was so contradictory to common sense, that no rational argument could be expected to avail. I repeat a mother's answer. "Four of my children have already died of the common small-pox; and, if my only remaining child should die by inoculation, I could never forgive myself."

II. The more opulent inhabitants of Leeds, Liverpool, and Newcastle, encouraged by the example of Chester, have, at several periods, bestowed inoculation

culatation on all the poor children of those towns, who would accept the favour, as you will find mentioned in the printed Correspondence, which accompanies this letter.

Some years ago, I had the honour to receive, from a committee of the Royal College of Physicians at Edinburgh, a letter of inquiry concerning the transactions of our small-pox society at Chester; with a view, I believe, of introducing similar establishments into Scotland: but I do not learn that any such measures have been hitherto accomplished in that kingdom: except, that last autumn inoculation was offered gratuitously to all the poor citizens of Edinburgh, who would accept the favour, by the Royal Colleges of Physicians and Surgeons: and a physician of Dumfries has sent me information, that the benefits of inoculation were successfully extended to the poor inhabitants of that town about three years ago. But, as far as I know,

neither in that nor any other part of Great Britain, except Chester, have any regulations been attempted, to exclude the casual small-pox.

III. Our society at Chester was a private voluntary institution, without any interference of Government, or of the City Magistrates, except as individuals.

IV. I will endeavour to compare the relative situation of Geneva and Chester, in regard to the difficulties that may occur at each place, in the execution of regulations to preserve their citizens from the casual small-pox.

I. I presume that the houses which you describe, containing, on an average, twenty-one persons, are generally divided into three, four, or more families, each occupying a different story; or, at least, separated from each other by an outer door, so as to have no necessary intercourse with any extraneous persons, except by an inclosed staircase;
in

in the same manner as the houses in the High-street of Edinburgh connect the families which they contain. This being so, my observations lead me to say, that a person in the casual small-pox may be safely allowed to meet others, liable to the distemper, even in the inclosed staircase, passing to the streets and publick walks, till the eruption is completed, that is, till the third or fourth day, after it begins to appear: but, unless the pustules are very few, not much longer.

The whole evidence produced in the INQUIRY and the CORRESPONDENCE, particularly by Dr. HEBERDEN, Dr. CLARK, and myself, uniformly proves, as far as it goes, that the distemper, till this period, is not infectious. On the contrary, a patient with many pustules, in the stage of maturation, meeting persons liable to the small-pox in a close staircase, would probably communicate the infection. I reason upon facts,

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which I have received on the best authority, from eye witnesses. One patient of mine was infected in the Rows, and another on the Walls of Chester, by a less intimate communication than would commonly happen by passing a person in a close staircase. (*Recherches*, p. 34. 86.).

The variolous poison probably renders the surrounding atmosphere infectious to a greater distance, in proportion to its quantity and virulence. Thus I should conjecture, that the malignant, and confluent kind is most infectious; not merely because it contains the largest quantity, but the most virulent sort of matter, and of the most offensive odour.

The inoculated is much less infectious than the casual small-pox, because it produces incomparably fewer pustules, and these generally dry away before they come to maturation; and because it very rarely produces a confluent kind,

kind, and still more rarely, with any symptoms of malignancy.

However, sometimes, though very seldom, the casual distemper generates but one or a few pustules: and such a case is, perhaps, not more infectious than one by inoculation, with the same number and size of pustules, which come to complete suppuration. Again, a case of the confluent, inoculated small-pox, which sometimes happens, may probably be as infectious as a similar case of the casual distemper. It is likely, though you, gentlemen, give no such intimation, that your inoculated patients, with numerous pustules, remain at home, during the period of maturation and exsiccation; otherwise I should apprehend that a person, liable to the distemper, might be infected, by meeting such a patient in a close staircase.

This comparative statement contains more conjectural observations than I wish to introduce into such disquisitions. However, it is illustrated and supported by the *whole* evidence which experience has taught us on this point; for inoculating matter is taken indiscriminately from a good kind of the inoculated or casual small-pox; and no body has discovered or suspected any difference in their degrees of virulence.

In the INQUIRY, the SKETCH and the CORRESPONDENCE you will find strong and I hope convincing evidence, to prove, that the sphere of infection of a small-pox patient does not extend beyond a very narrow circumference. The whole of this evidence is applicable to your particular situation, and shews, that the *Rules of prevention*, even in your closely built city, are adequate to their intention. However you will consider, whether, from the habits of the unthinking populace, an injunction should

should be inserted into the Rules "not
" to allow a patient, with numerous
" pustules, in the stage of maturation, to
" appear in the publick staircases." At
this period of the disease, such a restriction cannot be injurious to the patient.

2. Chester is a great thoroughfare from England to Wales and to Ireland; though much less so, I believe, than Geneva between the states which surround it. However, little difficulty is to be apprehended from this cause in either place. An infected person, indeed, may travel to a great distance, before any symptoms of the distemper are perceptible. During the establishment of our small-pox society, a man who had caught the infection in Scotland, was not attacked with the variolous fever till he arrived at Chester. But, the distemper chiefly spreads by the visits of relations and neighbours, who entertain no suspicion concerning the mischievous consequences of their domestic

mestick intercourse, and who could, without much inconvenience, when properly instructed, avoid all dangerous communications.

3. As your plan includes some of the surrounding country, it will, on that account, be more practicable than our's, which did not extend farther than a single town. You will often be able to stop the progress of an epidemick in the country, before it is brought into the city; and, on the contrary, suppress it in the city, so as to preserve the surrounding country from infection.

4. In the above circumstances, the difference of situation, in regard to the facility of checking the progress of the small-pox at Geneva or at Chester, is of little importance, one way or the other. But, if I am not much mistaken, your preventive regulations will experience the greatest advantages, in the

the more favourable disposition of your inhabitants.

In Chester, and, I believe, in most of the large towns of England the casual small-pox is almost constantly present. All the children of the middle and higher ranks of our citizens are inoculated in early infancy. The populace, very generally regarding the distemper as inevitable, neither fear nor shun it; but much more frequently, by voluntary, and intentional intercourse, endeavour to catch the casual infection. All the difficulties of our society proceeded from this strange delusion, and perversity of disposition. With us, the small-pox is seldom or never heard of, except in the bills of mortality; but there its devastation appears dreadful indeed! (See Percival's Works, vol. ii. 69—78; Recherches, p. 129; Sketch, p. 31, 138.).

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As the epidemical small-pox does not return to Geneva, oftener than once in five years, it seems probable, that such fatal visitations will spread a considerable alarm among all ranks of the inhabitants. As some families, even of the populace, may escape infection, at each of these epidemics, several of their children will be grown to an age, when they will justly dread the painful and dangerous sufferings to be expected from this destructive pestilence. Such impressions, it may be hoped, will produce habits of thinking and talking which will have the most beneficial influence.

This opinion is strongly confirmed by another circumstance. Both your and Dr. ODIER's letters declare, that although you have often inoculated at a time when there is no small-pox whatever in the city, and although the children so inoculated often resorted to the streets, the walks, and other publick places, before, during,

ring, and after the eruption, yet you never observed that they spread the contagion. The learned physician above-mentioned, says farther, "there never came to my knowledge an instance clearly proved, of the infection being communicated to a single individual, either in the streets or the publick walks."

These well-authenticated facts are, in many respects, highly instructive; and I refer to them on the present occasion, to say, they have persuaded me fully, that a salutary dread of the casual infection very generally prevails among your inhabitants, so as to keep persons who are liable to the distemper, at a safe distance from the infectious patients. For it is, I believe, generally allowed, that an inoculated patient, by a close approach, will communicate the casual infection; and if my creed be true, in one constitution of the atmosphere, as well as another. For these reasons, it seems highly probable, that
your

your endeavours to prevent the introduction and communication of the casual small-pox would be more easily and successfully accomplished, in Geneva, than in any large town in England. A dread of the distemper will pervade the remotest recesses of society, and open a thousand eyes to watch all irregularities, so as to assist and strengthen the *Inspector's* office, upon innumerable occasions.

V. In answer to your inquiry concerning the annual expence of our society at Chester, I transmit you a printed report of all our contributions and disbursements for four years: but apprehend that it will not enable you to form a probable estimate of the expenditure of your intended establishment. Our *Inspectors* were only the apprentices of surgeons and apothecaries, for whose services a small pittance was thought to be a sufficient recompense. The *Director's* office was executed

executed gratuitously. Both of these duties ought to be undertaken by medical gentlemen of respectable knowledge and character, whose authority would give great weight and influence to their instructions.

I have no doubt that physicians and surgeons of eminent professional abilities would offer their services to the publick without any gratuity, on this, as they readily do on other occasions. But mankind are too much disposed to think that what costs nothing is worth nothing. I would recommend that a compensation be assigned to the *Director* and *Inspectors*, adequate to the trouble and anxieties of their office, but that every neglect of duty should be censured with extreme severity. The compensation here intimated need not be pecuniary, or not exclusively so, but some increase of consideration, and respectability among their fellow citizens. In your well-regulated Republic,

lick, there are various degrees of privilege and honour, by which the inhabitants are admitted to the general Assembly, to the great Council, or to the Senate. Through want of knowledge in local circumstances, I cannot make any specifick proposal; but, if the principle I have ventured to suggest be just, you will have no difficulty to apply it in a proper manner.

At our first general inoculation in Chester, a small bounty was given to the poor families who were inoculated; but, for the reasons stated in the *Proceedings* of the society, it was afterwards discontinued as unnecessary and improper.

However, the reward bestowed on poor families for a faithful observance of the *Rules of prevention*, will probably be approved by the Noble Chamber of Health, and will appear to be highly expedient. It is not a recompense to
Fathers

Fathers and Mothers, for the preservation of their own children, but of their neighbours and acquaintance, from the casual small-pox.

Indeed, a reward, in one case, may be properly offered for the inoculation of a poor family; because it would be a publick as well as a private benefit. When an individual is attacked by the casual small-pox, among several liable to it, a premium to inoculate the remainder may be with advantage proposed. By this means, the quantity of poison generated, the period of its continuance, and consequently, the danger of communicating the infection to others, will be all greatly diminished.

In the separate printed report of the society in 1782, which is inclosed, you will find that the rewards given to the poor families in Chester for observing the rules of prevention, amounted in four years, to £54 : 18 : 0. To convey

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vey all the intelligence in my power on this point, I have examined the accounts of the society for the remaining two years. During this latter period, one hundred and thirty-nine double rewards of a crown (£34 : 15 : 0) for faithful and successful observance, and one hundred and thirty-six single rewards of half a crown (£17 : 0 : 0) for incomplete observance, of the *Rules*, were given by the society: consequently, the whole amount of the rewards, for the last two years, was £51 : 15 : 0. At that time Chester contained not much more than fifteen thousand inhabitants.

Whether this charge may be higher or lower than Geneva, in proportion to the size of the city, it might be difficult to form an accurate estimate. But, if your populace be as well disposed to the reception of such regulations as I have conjectured, and if in reality they entertain a proper sense of the

the mischief to be apprehended from receiving and communicating the casual distemper, it is highly probable that this article of expence will be comparatively much less, in a series of years, to your establishment, than it was to our's.

Near the termination of the quinquennial, or a longer period of exemption from the distemper, if one, or a few families only be attacked by the casual small-pox, and observe the *Rules* with such strict fidelity as perfectly to extinguish the contagion, an ample reward of such service would be the best œconomy.

It is only the poorest of the populace who will require any recompense for observing the *Rules*, and at a season too of distress, from the visitation of this loathsome disease, when motives of humanity, independent of every other consideration, would open the hand of charity. Your mode of thanking the

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families

families who observe the *Rules* gratuitously, should be pleasing to the individuals, and, as far as possible, should attract the pulrick attention.

VI. Lastly, you honour me, Gentlemen, with a question how far I presume it may be practicable to promote inoculation, and to hinder the propagation of the casual small-pox at Geneva. The only doubt I entertain on the subject arises from the uncertainty you express, whether any money can be raised for the purpose, either by private contributions, or by government. But, when I consider the character of the magistrates of Geneva, who are, I am well informed (by Dr. DE LA ROCHE) “particularly attentive to matters of direct publick utility, which they are generally very careful to prosecute;” when I consider the generosity of your government in supplying the expence of education to the children of all ranks of
your

your citizens; and when I compare these circumstances with the ardour you express on an occasion, which so highly concerns the preservation of their lives; I cannot doubt of your judicious and zealous exertions in a cause so truly meritorious.

If the management of this business be undertaken by your government, it will enjoy more authority and permanency, besides many other advantages, superior to a private society. Might your opulent citizens be invited to offer their voluntary benefactions to support such an establishment? Their humanity and their fears may induce them to give generous supplies for purposes so obviously beneficial to the community. However, the reluctance we observe in the subjects of all countries to make voluntary presents to their rulers, who usually take what they want by compulsion, may occasion some doubt how far you could rely on the

punctual continuance of such charitable contributions.

All things considered, I cannot but entertain very sanguine hopes that you, Gentlemen, will heartily undertake this business, and accomplish it in a manner that will be highly honourable and beneficial to your own state, and so as to become an example to the enlightened nations around, which sooner or later will attract their notice and imitation. Your establishment may encourage us to propose a revival of our private society at Chester, though our proceedings have been suspended for several years, by the unaccountable prejudices of the populace, who, I believe, have never expressed any wish to partake again of the benefits which were formerly offered them. It may also promote like institutions in other large towns in Britain; though most of them, especially those which are situated in this part of England, would have

have much greater difficulties to encounter than the inhabitants of Geneva. However, these difficulties, when aided by your example, may be easily surmountable, and would be the best preparatory steps to establish a general law, to include our whole island. And although it would be a much more arduous task to exclude the casual small-pox from any single town in England than from your Republick; yet, if the whole kingdom were to unite in the execution of such a measure, there can be no reasonable doubt that the casual small-pox might be exterminated, with much greater ease and success from our, than from your territories; the intimacy and the frequency of communication with foreigners, in proportion to the bulk of the people, is incomparably less in our, than in your situation. Such a ray of hope naturally kindles the warmest glow of patriotism in an Englishman's breast!

On this occasion, I cannot pass in silence, the high obligation and lively gratitude which I owe to your fellow citizen Dr. DE LA ROCHE. His translation, into the French language, of the INQUIRY how to prevent the small-pox, first revived my hopes on this subject, and incited me to write the SKETCH of a plan to exterminate the casual small-pox from Great Britain; in order to introduce general inoculation. Such thoughts had lain brooding in my mind for a long time; but I was afraid publickly to propose innovations so contradictory to the common opinion of mankind. When the INQUIRY had attracted the notice and approbation of a learned physician, so far as to honour it with a translation, and one of uncommon perspicuity and elegance, if a foreigner may presume to be any judge of the language, I began to hope the subject would obtain some portion of that attention from the intelligent part of mankind, which to me
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it had long appeared to deserve. I need not say what a pleasing confirmation of these hopes I have received, by a letter from so respectable a body as the Noble Chamber of Health at Geneva.

You, Gentlemen, are justly attentive to avoid every restriction that can possibly injure or alarm the precious liberty of the meanest individual. And I am clearly of opinion, that no coercive method will be required, to hinder the introduction or the communication of the small-pox. Exhortations corroborated by rewards, which indeed ought to be given, to the poorest families, and to them only, will, in general, be fully sufficient to accomplish this purpose. At any rate, no fault should be punished, but what is condemned by the publick voice. The fears of the people will carry you forward, as far as and probably farther than, may be necessary. In the OUTLINE of an establishment for Great Britain, some punishments

ments are proposed; but I am decidedly of opinion that none ought or need to be inflicted, except against those who wilfully and wickedly do wrong, and whose conduct is generally and loudly blamed by their neighbours. If any law be required to enforce restrictions, which are thought necessary, by the people in general, they would be sanctioned, with great unanimity, both by your Senate and general Assembly.

Before the establishment of our society, children in all stages of the casual small-pox often frequented our streets, rows, and walls, which are the publick walks of the town; because few feared or blamed this conduct. By the regulations of the society, our fellow-citizens were taught that such intercourse between the infectious and those liable to infection might have pernicious consequences; and during the several years which have elapsed since the cessation of our preventive

ventive measures, I have never seen such a patient in any place of publick resort.

In the letters of Professor WATERHOUSE (Recherches, p. 122, Corresp. p. 260.) you will find much curious and instructive intelligence to illustrate this point. Observe the powerful operation of fear in the free states of America. It enables their magistrates to enforce regulations a thousand times more oppressive than I have proposed, or than at all appear to be necessary.

It may be proper to recommend, as a regulation of police, that a ward for each sex should be prepared in a hospital, ready to receive all poor patients taken ill of the casual small-pox. The attendants of such wards, by easy instructions and habits, in the simple *Rules of separation and cleanliness*, would effectually preserve all the other patients from the danger of infection.

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This observation is fully warranted, by the perfect safety of two such wards in the Chester Infirmary, appropriated to the reception of contagious fevers, ever since the year 1783. Such a receptacle for patients in the casual small-pox would be quite as safe, cheaper and more commodious, than a pest house.

In regard to the mode of transacting the business of such an institution, the *Proceedings* of the Chester Society, and the proposed *Outline* of an establishment in Britain, will best explain my ideas. However, I have thought that a copy of the three kinds of printed certificates granted by the Inspectors might be of use to you, as they comprehend fully and minutely all necessary information. Firstly, They mention the facts where manifest transgressions of the Rules deprived the family of any reward :*

* " I saw the child (Elizabeth Stout) before all the scabs were entirely fallen off, playing with children liable to infection in Dee-Lane and Northgate-Street.

GEORGE VENABLES, Inspector."

secondly,

secondly, The circumstances of imperfect observance; where only the single reward was given:* and thirdly, The faithful and successful observance, where no neighbour or acquaintance was infected, which claims the double reward.† It was before mentioned, that, in two years one hundred and thirty-nine certificates for the double reward were granted: a fact of itself which shews the sufficiency of the *Rules*, and how perfectly they would preserve from in-

* "I have frequently visited the family of H. Jackson, while they were ill of the small-pox, but never discovered that they transgressed the *Rules of prevention*. But there has probably been some unobserved transgression of the *Rules*, as two children of their neighbours or acquaintance, have been attacked by the small-pox, either during the time it was in the said family, or within sixteen day after it had ceased therein, on the 18th of November, 1783.

W. CONNAH, Inspector."

† "I certify that no neighbour or acquaintance of Ann Dyson has been seized with the small-pox either during the time it was in the family, or for sixteen days after the last scab had dropped off, though three children, liable to the distemper, live within one door of the family.

W. CONNAH, Inspector."

fection

fection the inhabitants of any place, who were disposed to avoid it.

But the point which I recommend, Gentlemen, as of greatest importance, is to ascertain, with all possible accuracy, what *Rules* are *necessary* and are *sufficient* to preserve mankind from the casual-pox. Investigate with all care and diligence, the law, by which the variolous infection is communicated. The two principal problems which appear to require solution are, first, to inquire whether variolous miasms, in any case, adhere to clothes, so as to convey and communicate the distemper: and, second, to determine the distance from the poison, to which the atmosphere is rendered infectious.

1. The former point may be known most satisfactorily, from practitioners, who visit patients in all stages of the distemper, and, soon after, persons liable to receive the infection. Inoculators are
hourly

hourly employed in exactly these circumstances. Whenever any doubtful cases are reported, let the truth be investigated with the minutest accuracy, and impartiality.

2. Many will suspect that the air may be tainted to a greater distance at one season than another. My illustrious countryman SYDENHAM advanced an hypothesis, that peculiar constitutions of the atmosphere were favourable, and even indispensable, to produce the epidemical small-pox. This opinion has been confirmed by the most respectable medical testimonies; and the regular quinquennial Epidemics of Geneva will have increased the conviction of your most intelligent physicians in regard to this hypothesis.

As far as this notion prevails, it will have a pernicious effect on all your preventive measures. During the Epidemick, it spreads too general a despair
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to escape the infection; and, in the intervals, too dangerous a security. If you execute your intended plan of regulations to prevent, as far as possible, the introduction and the communication of the casual distemper, within your territories, with the zeal which you promise, the above-mentioned hypothesis will be brought to a decisive trial. You may recollect a case before referred to, in the INQUIRY (*Recherches*, p. 86.) where, *during an Epidemick*, three gentlemen's children passed within *half a yard* of a small-pox patient on the walls of Chester, without having caught the infection.

It will not be difficult to foretel that your endeavours to prevent the progress of the contagion must be attended with various success. Some families will be preserved from the distemper, while others, at a much greater distance, will be attacked. Unless this difference can be explained, by

by some direction of the wind, current of air, &c. you will fairly infer, in the latter case, that the *Rules* have not been faithfully observed.

The inferences which I have ventured to deduce from the cases, which are related upon my own knowledge, or upon the authentick testimony of others, may be admitted with doubt and hesitation, or perhaps may be totally rejected by many readers. But the transactions of a publick establishment, if accurately noted, and fully reported, will supply such abundance of authentick facts as to ascertain the truth beyond all controversy.

When you can say with certainty, and obtain general belief, that the small-pox can never be caught, but through some transgression of the *Rules of prevention*, innumerable advantages will be the result of such a creed. A very small part of society would willingly spread diseases and death among their neighbours. They do mischief

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merely

merely from ignorance of what is right and wrong.

This interesting question appears to be reducible to a narrow point: it is rather philosophical than exclusively medical. And I would recommend, that the physician whom you appoint the DIRECTOR, or, at least, that some of the committee of the Noble Chamber of Health, who conduct the business, may have a considerable portion of the knowledge, and the spirit, which have rendered your BONNETS, your SAUSURES and your DE LUCS so illustrious among philosophers.

Though I have hitherto been silent concerning that inquiry of your letter how "inoculation may be rendered more general among the people of the town and country," it is not because I think this part of your plan at all less important than the preventive regulations. But if you can preserve the populace, for a sufficient period

period of years, from catching the casual small-pox, I should hope that general inoculation would be the natural, if not the necessary consequence. Children of the poorest and most ignorant families would soon grow to an age when they could distinguish the great difference of misery and of danger between the casual and the artificial distemper; and they would demand the latter from their parents with an importunity that could not be resisted. When the eldest obtains this boon, the same benefit must be necessarily bestowed upon all the younger brothers and sisters in the same family. This motive has some influence in the villages nearest to Chester. The increasing fears of the rising youth will most effectually extend the benefits of inoculation to the lowest ranks of your inhabitants, if offered gratuitously, which undoubtedly ought to be done, at all convenient seasons. In these circumstances, I trust that the principle of self-preservation will be a sufficient motive to promote

L 1 2 inoculation;

inoculation ; if, however, it should fail, your wisdom and humanity may devise other methods to accomplish this indispensable part of your establishment. The letters of Professor WATERHOUSE exhibit an excellent picture of a country, where the casual small-pox is justly dreaded and inoculation eagerly sought by soldiers, sailors and every other class of people.

My anxiety to improve the occasion of your address, announcing your intention to form an establishment to hinder the progress of the casual infection and to promote inoculation, purposes which I have so much at heart, may, in this answer, have induced me to enter too diffusively into some disquisitions. But if any hint here suggested, at all contribute to the successful accomplishment of your beneficent intention, I shall easily bear the imputation of such faults, and you will readily excuse the superfluous remarks which
this

this long letter may contain. I shall wait with great solicitude, to receive intelligence whether, and with what success, the proposed regulations have been established.

The day on which this letter is dated, being so auspicious to the liberties and good order of the Republick, even a foreigner is induced to express his hope that Geneva may long enjoy a perfect exemption from the pernicious effects of civil discord; from the still more baneful influence of arbitrary oppression; and (what the subject of this letter naturally suggests) from the destructive ravages of the small-pox. That you, Gentlemen, as well as the other Magistrates of the Republick, may have leisure to employ all your thoughts and cares in rendering your fellow-citizens wiser, better and happier, is the sincerest wish of your respectful servant,

JOHN HAYGARTH.

P. S. Your letter as well as this answer, being well adapted to promote the intention of the sketch, I trust, that you will entirely approve of their publication.

APPENDIX

APPENDIX

TO THE CORRESPONDENCE.

Extract of the REMARKS on FEBRILE CONTAGION; by Mr. JAMES LUCAS, one of the Surgeons of the General Infirmary at Leeds, and Member of the Corporation of Surgeons in London. From the London Medical Journal for 1789, p. 260.

“UNABLE to succeed in promoting measures similar to the benevolent plan which has for several years been established at Chester, I have contented myself with occasionally practising Dr. Haygarth’s *Rules*; and have also recommended the practice to my acquaintance, should the small-pox become prevalent in their neighbourhood. Knowing that one of the gentlemen alluded to, who is a clergyman,* had not only taken much pains to establish such a plan, but had received from it much satisfaction, I requested him to transmit to me a narrative of his proceedings, which I shall beg leave here to insert.

* The Rev. Stephen Moore, Vicar of Brodsworth near Doncaster.

" In the month of May, 1786, I was informed
 " by the woman who nursed a child of mine, that
 " the small-pox had just begun to appear in a poor
 " family not far distant from her house. I desired
 " the family neither to associate with their neigh-
 " bours, nor to permit any one who had not had
 " the small-pox to come near them; and I also
 " called upon every family in the village to request
 " that they would be equally careful not to have
 " any communication with the infected house.
 " My directions were as punctually followed as I
 " could have expected; but the disorder proved
 " fatal to one out of two children in that family.
 " A child resident under the same roof, and a
 " young girl who imprudently visited the infected
 " corpse, were the only persons who caught the
 " infection, which I attributed to the precautions
 " used.

" In October, 1787, I was informed that a child
 " had been a day or two before brought from a
 " distant village extremely ill of the small-pox.
 " By an equal attention to Dr. Haygarth's instruc-
 " tions, every person escaped the contagion, ex-
 " cept a little child in the same habitation, al-
 " though it was a publick house, and in the center
 " of the village. In the beginning of December
 " following, I heard that the small-pox was very
 " rife in a neighbouring town, as well as in some
 " adjacent villages; to one of which I was told a
 " parishioner of mine was gone to nurse her grand-
 " son

“son who was dangerously ill of the small-pox.

“I was much alarmed lest the disorder should be
“brought amongst us, and thereby defeat a ge-
“neral inoculation, which I intended to offer the
“ensuing spring.

“Soon after, I was informed that the boy was
“dead, and that his nurse was expected home,
“where there were three children who had not yet
“had the small-pox, and who were also intended
“to partake of the proposed inoculation. I ob-
“jected to any of the family conveying my instruc-
“tions to the place where the disorder prevailed,
“although I found them desirous of using every
“precaution; but I requested that the nurse should,
“upon her return, put off the clothes she might
“then wear, at a relation's house, where there
“was no person capable of being infected; that
“her linen should be cautiously washed; and the
“rest of her apparel fumigated, and exposed to the
“air. By all these precautions being cheerfully
“complied with, I had the satisfaction to find that
“no one caught the infection.

“In the month of March we were again dis-
“tressed by a neighbour's child being brought
“from a distant school with a very malignant kind
“of small-pox, which soon proved mortal; yet,
“except two children in the same family, every
“one escaped being infected, although a person
“had taken his two children into a room where
“one

“ one of the patients was, with a view for them to
 “ take the disorder. From the information you
 “ had given me, I was in hopes the patient was
 “ not so far advanced in the disease as to commu-
 “ nicate infection, which proved to be the case;
 “ and the young folks were inoculated a few weeks
 “ after.

“ I had no sooner taken down the names of such
 “ children in the village as offered for inoculation,
 “ than I was requested by several persons to extend
 “ the same privilege throughout the parish. As
 “ such a plan exceeded my intended limits, I ac-
 “ quainted a noble Lord* with my proceedings,
 “ who immediately approved what had been done,
 “ and, in the most polite manner, requested that
 “ he might be allowed to be at the sole expence of
 “ executing a scheme which every family to whom
 “ he had applied had, not many years before,
 “ peremptorily refused.

“ Notwithstanding the unfavourable appearance
 “ of some of the children, including a few private
 “ patients, near eighty were inoculated, without
 “ even any apparent danger; whilst two, out of
 “ five, who caught the natural infection died. As
 “ I had three of my own children inoculated by
 “ the gentleman who undertook the general inocu-
 “ lation, and at the same time, I became not a
 “ little anxious that no want of success should

* Earl of Kinnoul.

“ happen

“happen from any failure in enforcing the necessary directions. Experience demonstrated the advantage of some person exerting himself in admonition as well as caution. I cannot too strongly solicit a similar attention in the clergy or principal persons of either town or country, being fully persuaded that, if such a mode was generally adopted, the most happy consequences would ensue; the lives and future health of the rising generation would be greatly preserved; the malignant effects of the disease would be so far moderated, as to render it as little dreaded as other complaints; if even hopes might not be entertained of eradicating it from our island.”

“I have endeavoured to establish in this town (Leeds) a plan for preventing the spreading of the small-pox, and also other febrile contagion; but although I obtained a promise of several contributions, yet I could not procure the necessary assistance and support for carrying it into execution. When I have been called to a patient in the natural small-pox, I have endeavoured to trace the introduction of the disease, and also to inculcate the preventive measures. In one instance I found that the disorder had been brought by a vagrant, and had been communicated to three families in the same street: by calling upon such families as seemed most exposed, and using every exertion in my power, a total check to the progress of infection, as far as I could learn, was the consequence.

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"The result of two general inoculations in Leeds* has been, that the small-pox has since been less frequent and less fatal; its introduction might more easily be traced, and the poorer class seems to have adopted a more advantageous method of treating the natural distemper.

"The possibility of carrying Dr. Haygarth's *Rules* into execution in villages requires so few exertions, and is evidently attended with such happy consequences, as to afford little doubt but the communication of the preceding narrative will have its due influence. There are bountiful persons in every place, who liberally dispense assistance to the indisposed and needy; and those who are accus-

• "One of these took place in the year 1781.

The number of inhabitants in Leeds, in that year, was 17,117; of whom 7475 (males and females) were under twenty years of age.

In the space of six months, in that year, 462 } persons had the
small-pox.

Of these recovered + 332

----- died - 130

In the next six months were inoculated - - 383

Of these recovered - 381

----- died - 4

The number of those who were still uninfected was found, on a survey, to be 700.

Two of the four, who died, evidently appeared, from the early commencement of the eruptive fever, to have received the natural infection previously to their being inoculated.

The general inoculation, so far from spreading the disorder, appeared to put an immediate check to its progress."

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tomed to such visits would seldom fail to gain early intelligence of the small-pox and any dictates of their's would be scrupulously observed from the best of motives, gratitude and affection. The experience and success of villages, in stopping the progress of infectious disorders, would tend to encourage societies being formed in populous towns for the same laudable purpose.

“ In the year 1779 I addressed a letter to the gentlemen who had the management of the poor in this town, recommending to them, in as strong terms as I could, a house of reception for such as might be seized with infectious fevers, and require assistance from the town. I was led to this step from having visited several patients who laboured under a malignant fever, and from observing that the same contagion continued for many months; that eighty persons died of the disorder in one year; that many who struggled through the disease died of other lingering complaints; that in two courts, or yards, forty persons were affected with the fever; and that some families had received ten shillings a week from the poor assessment; that such a scheme appeared likely to check the malignant tendency of the disease, and at the same time to be more economical than supporting such paupers at their own houses, where no expence could afford comfort, or that preservation which might be expected from a well-regulated temporary hospital.”

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The misery produced in poor families, by infectious fevers, here described by Mr. LUCAS, has long been an object of my anxious concern. I am glad to take this occasion to recommend, from experience, a plan of admitting such patients, into a hospital, and placing them in separate wards. This measure has been accomplished, ever since the year 1783, in the Chester Infirmary, without communicating the distemper, in a *single* instance, to any other patient in the house. The *Rules* which I recommended, to regulate such an establishment, have obtained the honour of a place in Mr. HOWARD's account of Lazarettos, &c. p. 209.

To describe the numerous benefits which result from this institution, would far exceed the bounds of a digression. It may, at present, suffice to say,

say, shortly, that no hospital can be secured from fevers, but by appropriating separate wards for the purpose: and plainly for the following reason.

Though typhous contagion sometimes appears to produce febrile symptoms, soon after a person has received it; yet, in other cases, it continues, in a *latent* state, for a considerable period. This conclusion I have formed, by an attentive consideration of the progress of fevers in the same and in different families, in various situations, and Epidemics. If so, patients admitted into hospitals, for other disorders, will fall ill of fevers with which they have been previously infected, and spread destruction around them, unless proper accommodations are provided for their removal into separate wards.

CONCLUSION.

CONCLUSION.

In this Correspondence, I have endeavoured to introduce a full and unreserved, but candid disquisition, upon the nature of variolous contagion. The principal questions to be determined, are, whether the *Rules of prevention*, and whether the *Regulations*, deduced from them, in the *INQUIRY* and the *SKETCH*, are, or are not founded on sound reason, and sufficiently extensive experience.

Every fact, and every theoretical doubt, suggested by my learned Correspondents, have been scrupulously submitted to the publick consideration. This method of discussing the subject

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has unavoidably occasioned some repetitions, and more prolixity than could have been wished. But I was particularly anxious to place within the reader's view, the *whole* evidence, which I have been able to collect, on both sides the question. In so momentous a concern, the imputation of superfluity appeared to be less blamable than the charge of deficiency. By the former error, indeed, the author's character may suffer; but it can do no injury to his cause. The minutest investigation is indispensably requisite, in all our attempts to discover any law of nature.

The character of my Correspondents is so generally known, and so highly respected by the medical world, that it cannot be advanced by the feeble praise of an individual. With several of them, I have not the honour of a personal acquaintance. And the testimony of my intimate friends cannot reasonably be suspected to proceed from

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partiality :

partiality: indeed, *their* objections have been most freely and positively stated.

One circumstance may require a particular apology. The complimentary expressions of politeness, or of friendship, so universally employed in the epistolary intercourse of modern times, could not always be suppressed, without disordering the context. On the one hand, I wish to derive no credit from these customary terms of approbation, when unsupported by facts: and on the other, I entreat the reader not to impute their insertion to any silly motive of vanity.

In the selection of letters, which I have received, as answers to the *Queries*, my rule has been to publish those only which contained *original* observations, to illustrate the nature of variolous infection. The short extract from Professor IRVINE's communication is the only

only instance where this rule has been transgressed. He was eminently distinguished for his chemical and mathematical knowledge. As the theoretical arguments essentially depend upon these sciences, I solicited his unreserved opinion, and received his permission to publish it : for the satisfaction of such readers as cannot acquire their own complete conviction on these questions. It was inserted as a balance, in some measure, to the positive condemnation of the theory, by some of my very ingenious Correspondents, but chiefly, perhaps, as a token of respect, to the memory of a departed friend.

I have not the remotest expectation or wish to silence objections by quoting authorities : being fully aware, that, in this fortunate land of perfect liberty, there is no method so likely to excite doubts and opposition. Such doubts introduce the most useful disquisitions.

Of theoretical argument enough, or more than enough, has already been employed, on both sides the question. For the truth of the doctrine advanced concerning variolous contagion, I wish fairly to appeal to the impartial testimony of facts. As these may be so easily obtained, and can so fully decide the question, it is hoped that no opposers of the doctrine will argue from any fanciful hypothesis. Let those who disbelieve the authenticity of the cases reported by others, make their own observations.

*Segnius irritant animos demissa per aurem,
Quam quæ sunt oculis subjecta fidelibus, et quæ
Ipse sibi tradit spectator.*

In a short period of time, the distemper appears in so many places, at least in this neighbourhood, that opportunities to obtain personal knowledge of facts, which would determine the disputed points, may be every where

where found, by all who are desirous and will take the trouble to seek them.

On the evidence of original and independent observations, communicated by my Correspondents, I shall review the chief propositions and practical rules, with all possible brevity.

I. In the INQUIRY, I had argued that when *three* or more persons are exposed to the variolous infection, for the first time in their lives, they very rarely *all* escape the small-pox. As many important conclusions rest upon this argument, the reader will excuse my solicitude to establish it, upon a firm foundation.

Proportion
of mankind
liable to
small-pox.

I. On the authority of respectable authors, it was assumed as a preliminary observation, in the INQUIRY, that not more than *one* person in *twenty* is naturally exempted from the distemper. To elucidate this

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point,

point, some new evidence is adduced, though confessedly too little to determine what may be estimated the mean proportion. In the introduction to the SKETCH (p. 32.) it is related, that, all the Cheshire and Lancashire Militia, amounting, together, to about twelve or fourteen hundred men, had passed through the small-pox, except about one in twenty; although several of them might never have been exposed to the contagion.

Again, the inoculations performed under the direction of Dr. CLARK, at the Newcastle Dispensary, and of Dr. WOODVILLE, at the small-pox hospital, in London, supply some accurate *data* to our purpose (p. 397.). Not more than twenty-two were incapable, of receiving infection, by inoculation, out of eight hundred and thirty-three; or only about one in thirty-eight. As far as the testimony of these cases is conclusive, it appears, that the proportion

proportion of mankind liable to the distemper is greater than I had stated. The force of my argument, thence deduced, is consequently increased.

2. Assuming the proportion of *one in twenty* as a *datum*, it was calculated, by an eminent mathematician, Mr. Dawson, that, if *two* persons, who were never, previously, near the poison, had, on a particular occasion, been exposed to it, they would not both escape infection, it is probable, in the proportion of four hundred to one; that, if *three* were so exposed, it is probable, in the proportion of eight thousand to one, that they all would not remain uninfected.

It must be confessed, however, that as this part of the subject is very imperfectly investigated; the *data* may be erroneous: no author having attempted to determine what proportion of mankind has a *temporary* incapacity to

CONCLUSION.

receive the variolous infection. Dr. WATERHOUSE has produced a clear example of it "in a noted attendant of a small-pox hospital," (p. 278). However, instances of this kind are so very rarely heard of, that they probably will have little effect on the calculation.

Again, some families may be less liable to the small-pox than others. An intimation is given, in the Correspondence, that such a family exists in Geneva: but the fact has not been ascertained with such precision as so rare and valuable a circumstance would deserve.

To determine the accuracy both of the calculation, and of the *data*, upon which it is founded, by actual observation, the third query was proposed. "Did you ever know *three* or more persons, at the same time and place, all escape the small-pox after being certainly exposed, for the first time, to the variolous infection?"

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If the *data*, or the calculation, above stated, be false, this query would discover the mistake, in numerous instances. The question is thus brought to the decision of plain facts, which lie open to the observation of every medical man, and of any other person who sees the small-pox in any family. The query was addressed to the publick in 1784, and to my intimate friends in 1778. Beside the respectable testimony of my Correspondents, above published, I have received evidence, from many other practitioners, both by letter and conversation, who uniformly answered this query in the negative.

The only exceptions, which have been reported to me are the cases related by Mr. Horne (p. 400.). These are very judiciously stated, and with such minute accuracy as to satisfy every doubt: unless, indeed, it be possible to suspect that the parents had been guilty of asserting a falsehood, for the sake of obtaining

obtaining the reward, given to inoculated patients. The failures of inoculation intimated in the Correspondence (p. 303.) will hardly be admitted as evidence to determine the query: they may be fairly explained by the mouldy and effete state of the variolous matter. The want of success in communicating infection at Boston, mentioned by Professor WATERHOUSE (p. 278.), might possibly be occasioned by a like cause; but the circumstances are not related so minutely as to enable us to draw any positive conclusion. When this point of inquiry shall excite more general attention than it has hitherto obtained, several other examples of failure will probably be recorded: and the subject will become better understood. In the meantime, the whole evidence which has come to my knowledge, Mr. Horne's solitary instance excepted, fully and decidedly supports the conclusions formerly submitted to the reader's consideration. The argument hence derives
great

great stability. Therefore, when three or more persons, together, have escaped the small-pox, we are warranted to infer, upon evidence of high authority, that all of them have not been exposed to the contagion.

II. The Correspondence has supplied some excellent facts to ascertain the period during which the variolous poison continues *latent* in the body.

Latent period
of infection.

1. From the concurrent testimony of Doctors WOODVILLE, CLARK, and J. CURRIE* the eruptive fever sometimes commences so early as the fifth day after inoculation. And Dr. CLARK has very minutely and judiciously related three cases, wherein it remained so late as the sixteenth, seventeenth, and twenty-third day.

2. To the small collection of facts, recorded in the INQUIRY, to determine the *latent* period of casual infec-

* 397, 460.

tion,

tion, five cases are added of unquestionable authority, by Drs. PERCIVAL, CLARK, BINNS, and J. CURRIE.* It is true, that our whole stock of experience is too small to warrant any satisfactory conclusion, as one of my correspondents has justly remarked, and I had previously confessed. However, as far as these cases bear evidence, they confirm an opinion which I formerly ventured to advance, that *the casual is longer, but not much longer, than the inoculated period.*

From the table (p. 397.) which states the cases of eight hundred and ten inoculated patients, it appears that five hundred and nineteen of them became feverish *before the ninth day*, and two hundred and ninety-one on or after the *ninth day*, that is, in the proportion of more than five to three.

* p. 234, 420, 422, 442.

Whereas,

CONCLUSION.

541

Whereas, not one of these *five* or of the former *ten* cases of casual infection had symptoms of the variolous fever earlier than the *ninth* day : and, in all, except one patient, it commenced from the *tenth* to the *fifteenth* day.*

This investigation is necessary to ascertain the proper period required for the performance of quarantine.

Another practical conclusion will be the result of this knowledge. Hence we learn, after what intercourse with small-pox patients, inoculation may be successfully performed. Dr. CLARK's cases so fully illustrate this point as to require no farther explanation. His and my inferences, on this head, are

* Dr. Woodville has supplied me with another fact to the same purpose, in a letter dated May 5, 1792. " A
" soldier died at the small-pox hospital. A boy, who
" was sent for his clothes, became infected, and was
" afterwards under my care at the hospital. Upon
" inquiry, I found it was upon the *eleventh* day after
" taking away the clothes that he began to complain."

confirmed

confirmed by the facts related by professor WALL.*

Clothes not rendered infectious by miasms.

III. The intelligent reader will be surprised that so very few cases have been produced by my Correspondents where clothes, previously exposed to variolous miasms, were even *suspected* to communicate the distemper.

The only examples of such a mode of infection which I have not endeavoured to explain in the Correspondence is the observation of Dr. ODIER (p. 430.). "In answer to the fifth query," he says, that, "cases where infection was communicated by these means," namely, by persons and clothes previously exposed to the miasms of a small-pox chamber, "are not uncommon." His letter arrived, when I was in haste to finish this volume, and, without delay, it was sent to the press. But fresh matter

* p. 250.

and

and other causes having delayed the publication, I transmitted to him a copy of the printed sheets, together with a letter which requested some explanations on certain points of our discussion. One of these was as follows.

p. 430. "D'un coté &c. pas rare."

'By the whole tenour of my book, you will see the importance of such cases; and, if you can collect any that are well authenticated, I request the favour of you to communicate to me an exact detail of them. But remember that, when the power of witchcraft was the publick creed, numerous undoubted facts, were daily reported in its support.'

I was favoured with Dr. ODIER's answer, dated 1792, May the 4th, of which the following extract is applicable to the present disquisition.

"Quand

“ Quand j’ai dit, en reponse à votre 5 question, que les exemples de communication par le moyen de personnes ou d’habits antérieurement exposés aux miasmes de la Petite-verole, ne sont pas rares, je n’ai pas voulu dire autre chose, si ce n’est que des personnes ou des habits qui avoient été en contact avec des malades de Petite-vérole avoient frequemment communiqué la maladie, et plus frequemment encore, à ma connoissance, ne l’avoient pas communiquée. Mais je crois, comme vous, que les cas de communication peuvent être expliqués en supposant que ces personnes, ou ces habits étoient infectés de quelque portion de pus, ou de croutes varioliques, qui s’y étoient attachées, et dont le transport a pu répandre l’infection. J’ajoute seulement que dans ces cas là, je n’ai pu acquérir la preuve de l’exactitude de cette explication, et qu’on voit si frequemment des exemples de communication, qui jusqu’ à présent ont échappé

échappé à l'attention des observateurs, & qui peut être s'ils étoient connus exigeroient quelques précautions nouvelles. Supposons, par exemple, qu'on put prouver que, dans quelques cas, le pus ou les croutes varioliques, transportées par des mouches, ou d'autres insectes, ont communiqué la maladie à de grandes distances, il suivroit de cette observation qu'il seroit convenable d'envelopper les malades ou leur lit d'une gaze qui les mit à l'abri des insectes."

"When I said, in answer to your fifth query, that examples of communicating the infection by means of persons or clothes, previously exposed to variolous miasms, are not uncommon, I only intended to say, that persons or clothes, which had been in contact with patients ill of the small-pox have frequently communicated the distemper; but, more frequently still, to my own know-
N n ledge,

ledge, they have not communicated it. And I believe, as well as you, that such instances of infection can be explained by supposing that these persons and these clothes were rendered infectious by some portion of variolous pus or scabs, which adhered to them, and, thus conveyed, was capable of communicating the infection. I only add that, in these cases, I have not been able to obtain exact proofs of such an explanation; and that one frequently sees examples of infection escape the attention of observers, and which, perhaps, if known, would require some new precaution. Let us suppose, for instance, one could prove, in certain cases, that the variolous pus or scabs, transported by flies or other insects, have conveyed the distemper to a great distance; it would follow, from such an observation, that it may be expedient to envelope the patients, or their

their beds with gauze, so as to render them inaccessible to insects."

Dr. ODIER's knowledge is very generally respected by the medical world; and this explanation clearly proves that his candour is not inferior to his knowledge. The testimony of all the other letters will have great weight with the candid reader. But the negative proofs exhibited by Dr. AIKIN's observations for twenty, Mr. HENRY's for forty, and Dr. CLARK's for eighteen years, have peculiar force. These gentlemen during the above periods, have had frequent, and intimate intercourse with both the casual and inoculated small-pox. The two former, by their very ingenious criticisms on the theory which suggested the above conclusion, had turned their thoughts, particularly, to this subject. When a medical observer has a theory to support or to refute, he acquires peculiar sagacity of discernment: yet they candidly and

N n 2 explicitly

CONCLUSION.

explicitly confess, that they never knew an instance where clothes exposed to variolous miasms conveyed infection to others.

In the INQUIRY, as well as in my answers to Professor WATERHOUSE, I have taken some pains to prove that negative are much more conclusive than affirmative proofs of infection. Two observations in the Correspondence, by Professor WALL and Mr. HORNE* shew, in a still clearer light, the fallacy of affirmative evidence. The sight of a patient, or the near approach of clothes, previously exposed to variolous miasms, seemed to have *instantaneously* generated the small-pox. These gentlemen and all the medical world agree in the delusive appearance of these facts. It is universally allowed that no mode of variolous infection produces the distemper at an earlier period than

* p. 257, 405.

the

the fifth day. Yet cases of this nature are peculiarly imposing. When an effect succeeds a cause *immediately*, it conveys to the mind much stronger conviction, than when it succeeds at a distance of time. Mr. HORNE's single case (p. 405.) has more the semblance of probability than any or than all the facts which have been supplied by my learned and sagacious Correspondents in support of the old, and universally received doctrine: yet every medical man will allow it to be utterly incredible, that fear, or infection, in any shape, should instantly produce symptoms of the small-pox. This and other cases of the same kind had undoubtedly suffered a previous, but unknown mode of infection.

IV. That the variolous infection extends but to a little distance from the poison, is a proposition, which the *Rules of prevention* require to be accurately true; and the new evidence pro-

Narrow
sphere of in-
fection.

duced in this volume clearly establisheth its truth.

The experiments of Dr. O'RYAN on this point are decisive. No theory, no hypothesis, no sophistry can possibly elude their force.

The fact related by Mr. DAWSON confirms, incontestibly, the same doctrine. He has informed me that John Airey, the young man who died at Sedbergh on the twenty-eighth or twenty-ninth day of the confluent small-pox, was buried on the fourth of January 1778. For three weeks preceding that event, he was undoubtedly in a highly infectious state. During this period, according to my meteorological register at Chester, the wind blew from the east, north-east, north, north-west, and the south. The atmosphere was clear, cloudy, rainy, hazy and foggy. And the register of the Royal Society proves that, in London,

don, the wind, during this time, blew from points at or near east, west, north and south: that the atmosphere was fine, cloudy, rainy, and *foggy*. As the wind blew all round the compass, both at London and Chester, we may presume that it was not less variable at Sedbergh. And the atmosphere was probably *foggy* at the last as well as the former towns, a circumstance, which one of my very ingenious Correspondents thinks favourable to the propagation of contagion.

Let the reader reflect on the circumstances of this instructive case. For three weeks, there is a constant emanation of variolous miasms from a large quantity of confluent pustules, in a narrow and much frequented street of a market town, where the distemper had not been epidemical for the preceding seven years. Persons liable to the small-pox must have approached within a few yards of the poison, many hun-

dred times, without receiving the infection.

What Dr. PERCIVAL observed,* in his own house, and in regard to his own child, shews, in the most convincing manner, the narrow limits of variolous and other contagions.

Mr. HENRY's testimony† manifestly establisheth the same doctrine. The small-pox spreads in one quarter of Manchester, while, in other parts of the same town, none or but a few are infected, though inhabited by many persons liable to the distemper, who escape it for many weeks or months. This is the true representation of all large towns, including the villages which surround them, nearly at all times. The small-pox is seldom or never absent; and yet never seizes all

* p. 227.

† p. 370.

who

who are liable to it in the same street, at the same time. If the wind could convey the pestilential effluvia so as to infect persons who inhabit an adjoining street, or even of an adjoining house, the distemper must spread inevitably and speedily through the whole town. Whoever duly considers facts of such magnitude and such notoriety cannot fail, one would think, to receive full conviction.

The method employed on board slave ships to prevent the propagation of the small-pox among the cargo, as related by Dr. J. CURRIE, is highly instructive. In the instance recorded, as eight persons had caught the casual infection, before the distemper was discovered, it is sufficiently manifest that the atmosphere, at that time, was capable of variolous contamination. Yet, in this very constitution, eight adult patients in the casual small-pox were placed, for several weeks, during

the day, within twenty feet of the air which was breathed by two hundred slaves, who were supposed liable to receive the infection: and indeed within sixteen feet of it, when the latter were standing or walking. Farther, by looking at the situation of the main top-sail of a ship when expanded by the wind, it manifestly appears that the largest portion of the air which blows over the round top, where the patients were stationed, is directed *downwards*, so as to approach still nearer to the slaves upon deck. No experiment made on purpose could have been better adapted to determine the medical question; when we consider the numbers who were capable of communicating and receiving the infection, together with the duration of the trial. And yet my truly respectable Correspondent mentions this as only one among many instances where so narrow a separation, when the weather allows it to be accomplished, always successfully prevents the communication of infection.

Comparing

Comparing these with the other facts before related, we must be fully convinced of the very narrow limits, to which the sphere of infection extends, either below or on the same level with the poison. How high the infectious quality may rise above it, I have not been able to determine by facts: but I cannot suppose that air would become lighter, by the addition of variolous serum, pus or scab, which are all heavier substances than air. However, in considering this question, I have cautiously avoided to deduce conclusions from theoretical arguments: these have been employed merely to suggest the observation of facts.

How far the variolous miasms ascend above the patient, may, I hope, be ascertained in a satisfactory manner on the following occasion. When the African slaves are attacked by the small-pox, at sea, they may probably be sometimes separated from the rest of the cargo

cargo in some apartment under deck. In this case, the question may be fairly decided, by observing whether persons liable to the distemper, upon deck, escape or catch the infection. I entertain hopes that this point will be soon and clearly decided by Dr. J. CURRIE. In a letter from him, dated the fifth of July 1792, he says, "In regard to
 "the question you have asked me,
 "whether the variolous infection can
 "be prevented from spreading on board
 "African ships, by putting the in-
 "fected into another apartment be-
 "tween decks, on the same floor with
 "the others, I have heard of its being
 "attempted, but have not been able
 "to procure evidence of its having
 "been successful. Such an instance
 "would indeed be much to your pur-
 "pose, and I will continue my in-
 "quiries."

The extract from a paper by Mr. LUCAS, a respectable surgeon of Leeds
 in

in Yorkshire, published in the London Medical Journal for 1789, and reprinted in the Appendix, contains some observations which are applicable to the present disquisition. It appears, from this account, that the *Rules of prevention* recommended by a clergyman to his parishioners, stopped the contagion in May 1786; in October and in December 1787, and in March 1788. It deserves particular notice, that small-pox patients situated in a publick house, and in the centre of a village, were prevented by these means from communicating infection to their nearest neighbours. And the facts recorded by Mr. Lucas, from his own personal knowledge, so plainly confirm the same doctrine, that the intelligent reader can require no explanation or application of the argument.

V. That some peculiar constitutions of the atmosphere are requisite for the propagation of the plague, the small-pox,

No epidemical constitutions of the atmosphere.

pox, and other epidemical distempers, is a doctrine which leads to such erroneous and pernicious consequences that, I hope, the reader will excuse a few additional remarks on this point. It is introduced on all convenient occasions as an answer to the plainest facts. This invisible and incomprehensible agent comes and goes, exactly as the case requires. It is a gratuitous supposition to solve all appearances, a mere hypothesis unsupported by any theoretical reasoning whatsoever, or even by a plausible analogy deduced from chemistry or any other branch of natural philosophy. It cannot, by any modification, be made consistent with facts. When an Epidemick spreads or stops, the cause of these events cannot be attributed with more reason, to a change of the atmosphere we breathe, than of the bread we eat, or of the water we drink. Twenty other hypotheses of equal or nearly equal plausibility might be easily invented. But
all

all of them, as well as the illusion, which, for above a century, has led the medical world into the most pernicious errors, might be refuted by an impartial appeal to the progress of contagion.

As the train of regular Epidemics at Geneva appeared to confirm the hypothesis, that peculiar constitutions of the atmosphere are favourable to the propagation of the contagion, I requested some explanations upon this head, in my letter before-mentioned, to Dr. ODIER. On this, as well as the former subject, his answer was written in the true spirit of a candid inquirer after truth. His observation plainly confirms the doctrine I have endeavoured to establish in both the former and present publications. “ Je crois
“ qu’il ne seroit pas difficile de démon-
“ trer que l’état de l’atmosphère n’y
“ a aucune part. Car dans les bourgs
“ et les villages qui nous avoisinent,
“ les

CONCLUSION.

“ les années Epidemiques ne font pas
 “ les mêmes que dans la ville, quoiqu’ils
 “ soyent situés sous le même ciel, &
 “ exposés aux même vicissitudes de
 “ l’atmosphère.” “ I believe it would
 “ not be difficult to prove that
 “ the state of the atmosphere is in no
 “ respect the cause. For the villa-
 “ ges and towns which surround us do
 “ not experience the same epidemical
 “ years as this city, although they are
 “ situated under the same heavens, and
 “ exposed to the same vicissitudes of
 “ atmosphere.”

SYDENHAM conjectures that some
 effluvia, issuing from the bowels of the
 earth, produce Epidemics. There is
 some semblance of consistency in this
 hypothesis: if true, it might advance
 one step toward a solution of the diffi-
 culty, by discovering a local difference
 in the atmosphere.

But it has never yet been pretended
 that any such vapour was perceived.

Yet

Yet every part of the earth must be capable of furnishing it; as no portion of the whole habitable globe has been discovered, where the air could not propagate the small-pox. The inhabitants of the hottest, of the coldest, of the wettest and of the driest regions of the world are, as far as hath been ascertained, equally liable to receive and to communicate the variolous infection.

Hence we may safely conclude that the slight variations of the same climate, and the same season, must be altogether insignificant and nugatory. What important difference of atmosphere can be imagined to exist, for weeks or months together, in two neighbouring villages, or in the adjacent streets of the same town? This remark is plainly applicable to the propagation of the plague and other infectious distempers. Yet the latest and most respectable authors continue

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to

to be misled by this groundless hypothesis.

In a letter from Dr. WATERHOUSE, dated the eighth of December 1791, he says; "there has not been a single person seized with the small-pox in Boston or its vicinity, for these eighteen months." Hence it appears, by comparing this, with the Doctor's letter of October 18, 1788, that the small-pox brought from London to Boston about eight months preceding the former time, was suffered to infect two persons only in that town, and but a few in the neighbouring villages. So striking a fact pleads for the cause, which I have undertaken, with the most persuasive eloquence.

Are the Rules
sufficient?

VI. Only three of my Correspondents have stated any doubts concerning the sufficiency of the *Rules of prevention*. Those of Dr. AIKIN and Dr. WATERHOUSE, I have already discussed

cuffed in my answers to their letters. Dr. ODIER's objection merits attentive consideration. He doubts whether the *Rules* contain all the necessary restrictions, because persons are sometimes attacked by the small-pox, when no specifick cause of it can be assigned.

In the present, as well as the former work, many facts have been adduced to ascertain the circumstances, when the variolous poison does, and when it does not communicate infection: both these points seem to have been determined with some degree of accuracy. But, though we may be perfectly acquainted with the nature of a cause, yet we cannot comprehend, in every particular instance, how the effect is produced.

Thus, when a house has been burned down, we entertain no doubt that it must have happened by the application of fire to its combustible materials. In

most instances, this cause can be clearly ascertained. But houses are frequently discovered in flames, where no probable conjecture whatsoever can be given, in what manner such a misfortune was produced; though the whole neighbourhood unite their intelligence and their sagacity to investigate the cause. Yet fire is incomparably more distinguishable, and dispersed with a much less degree of carelessness than the variolous poison. One who duly considers these circumstances, cannot reasonably require any other than the known cause of infection; though it may, in particular instances, elude the most attentive researches.

If this explanation together with those which were before given, be thought satisfactory answers to all the objections which have been stated against the *sufficiency* of the *Rules of prevention*; it will follow, that the unreserved approbation of them by my
other

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other learned Correspondents will be received with undiminished force.

But, though the *Rules* may be thought fully adequate to the purpose of preventing the distemper, yet when applied, they will require and admit of many improvements. Thus, all kinds of furniture bedaubed with variolous matter are directed to be washed. But in some cases, their contamination will be more easily prevented, than removed. Dr. PERCIVAL judiciously observes that the feathers or flocks of beds, and pillows could not be conveniently washed. A skin or even a blanket spread under a patient ill of the malignant small-pox would preserve the bed and pillows from the pus, ichor, &c. This instance will be a sufficient illustration. Minute details, which in practice will be required and easily introduced, are here purposely avoided. My sole view, at present, is to draw an accurate *outline*.

VII. In

Are any of
the restric-
tions super-
fluous?

VII. In a medical view, the *Rules* impose some restrictions not absolutely necessary, as is fairly argued by some of my Correspondents. How far these may be safely relaxed in their execution, unless under the immediate superintendence of accurate inspection may still be uncertain.

We may perhaps properly say, Rule first, "suffer no person, who has not
"had the small-pox, to come into the
"infectious" chamber; instead of the
"infectious house."

And Rule second, "No patient, after the complete eruption
"must
"be suffered to go into the street, or
"other frequented place," so as nearly to approach any person liable to the distemper.

As some objections have been intimated, that the proposed measures of prevention might be an impediment to inoculation,

inoculation, it may be proper to observe that such doubts are wholly unfounded. By the uniform tenour of the doctrine which I have attempted to establish, that the sphere of infection is circumscribed within very narrow limits, it is manifest that inoculated patients may safely be allowed the benefit of fresh air, as far as it can possibly answer any salutary purpose. The general inoculations of Chester, Leeds, Liverpool, Newcastle, &c. were performed in consequence of this doctrine. These, in no instance, increased, in some checked, and in others suppressed the casual distemper.*

As the foundation of the whole plan, which I have ventured to submit to the publick consideration, so essentially depends upon the accuracy and the truth of the *Rules of prevention*, it is hoped that the reader will excuse my great solicitude to prove that they compre-

* Inquiry, p. 188; Sketch, p. 453. 524.

hend

hend every necessary and no unnecessary restriction.

To the utmost of my power, I have thus attempted to establish practical principles for the prevention of the small-pox. I can honestly assure the reader that the numerous facts related both in the former and present publication, are exhibited to his view without selection, colouring, partiality or concealment. However, I would earnestly recommend a farther and independent investigation of the question, to some medical philosopher, who might employ his time and genius, on this subject, with the greatest honour to himself, and the utmost advantage to mankind.

THE END.

P. S. While this sheet was in the press, I received a letter from Dr. Waterhouse, dated the 20th of November 1792. He requests that two observations may be added to his Correspondence.

1. For six months, the small-pox had been epidemical at Boston. After twenty families were casually infected, general inoculation commenced, with unusual hurry and alarm. In all, nine thousand one hundred and fifty-two were inoculated; eight thousand of them in four days; and above another thousand in less than a fortnight. In numerous cases, the infection was not communicated. Some, in almost every family, were inoculated four or five times; yet few, very few of them took the disease in the casual way, although many were in the same room with those who had it full out upon them.

He expresses great solicitude publicly to declare that he had become

P p a profelyte

a profelyte to the new doctrine, that the variolous contagion was limited to a narrow sphere, and that, during the Epidemick, he observed no instance, where the distemper was conveyed by the wind to a distance, similar to what he had reported in his former letters, on the authority of others.

2. Remarks too digressive from the subject of this publication had been introduced into our Correspondence, on the mode of education at the two Cambridges of Old and New England. With true patriotick zeal, Dr. Waterhouse explains, in detail, that the students of the American University, employ four years, with great assiduity, in acquiring a knowledge of ancient and modern languages, mathematicks, history, philosophy, &c. Their plan of education appears to comprehend the most useful and important branches of academical erudition.

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